Essays on Gender, Intra-Household Allocation and Development

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Abstract

This dissertation studies gender, intra-household allocation and development. Industrialization and globalization has expanded opportunities for women in developing countries to work in manufacturing and service sector jobs often located outside their villages. The first chapter of this dissertation studies whether such job opportunities can lead to socio-economic changes for women, particularly with regard to marriage, fertility and empowerment. The second chapter examines the impact of a large public workfare program targeting rural households in India on children. In particular, we study the impact of time use by the youngest and oldest children in a household as adult time use changes in response to new work opportunities. The final chapter of this dissertation studies the impact of age of marriage on female mobility and autonomy in rural India.

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To my Ammamma and Appu

Introduction

In many developing countries, women traditionally lag behind men in multiple economic and social dimensions such as education, economic opportunities, decision-making and political representation. Economic growth and public policy initiatives are expanding the opportunities available to women, and this has the potential to change gender roles and improve the status of women, which, in turn, can affect development. This dissertation studies gender, intra-household allocation and development with a focus on India where conditions for women remain poor despite recent economic growth.

The first chapter of this dissertation examines whether longer tenure in the formal sector affects female empowerment, marriage and fertility decisions. Women in developing countries are starting to join the workforce in greater numbers, and it has been argued that such exposure can lead to improved outcomes for them. In this chapter, I exploit plausibly exogenous variation in duration worked from a natural experiment created by a large Indian textile firm's decision to replace fixed-term contracts with daily employment contracts. Using administrative data from this firm, I find that the more time women were exposed to a fixed-term contract, the longer they stayed in the formal labor market. Surveying 985 workers about 4.5 years after they first entered the textile industry, I find that the women who worked longer delayed marriage, without any detrimental effect on eventual spousal quality. A longer duration of employment also translates to reductions in desired fertility. Further, there are strong spillover effects within the family, as age of

marriage increases for younger sisters and school dropout rates decrease for younger brothers. I find evidence that an increase in female empowerment and autonomy is a plausible channel for these effects. These findings provide new information on the impact of duration of employment outside the parental village for young women in rural areas.

The second chapter of this dissertation, which is joint work with Mahnaz Islam, examines the impact of the National Rural Employment Guarantee Act (NREGA), a large rural public workfare program in India, on intra-household allocation of time and labor supply. Particularly, we focus on the impact of the NREGA on schooling and employment outcomes for children. We use several rounds of nationally representative cross-sectional data and panel data for three states from the National Sample Survey (NSS) in India. The NREGA offers 100 days of guaranteed work to adults from rural households with the intention to help households smooth consumption during lean agricultural seasons. Providing employment opportunities to households can affect intra-household allocation of time and resources by changing income and bargaining power. We use the phased roll out of NREGA to different districts and measure the difference-in-difference between districts that received the program early relative to those that received it later. In our analysis we look at the impact on children when adults take-up NREGA work. On one hand, additional income in the household can increase resources spent on children's education and reduce child labor. However, if wages in the economy increase or adults take-up new jobs, child labor could increase. Our results show an increase in time spent on education for younger children and an increase in time spent working outside the household for older children.

In the final chapter of this dissertation, I study the impact of age at the time of marriage for women in rural India on female mobility and autonomy. I study the causal effect of delaying marriage using age of menarche as an instrumental variable. I find that

while women who get married later get more education, they do worse in terms of later life outcomes related to female autonomy and mobility. For literate women, delaying marriage increases spouse′s education and long run monthly per capita consumption, while the opposite is true for illiterate women. However, delaying marriage increases the likelihood that the spouse′s family′s economic status at the time of marriage is worse than their own family′s economic status. This suggests that education mediates some of the costs women face from delaying marriage, but, some costs in terms of female autonomy may remain since beliefs on gender roles tend to persist across generations.

Chapter 1

Work and Women's Marriage, Fertility and Empowerment: Evidence from Textile Mill Employment in India

1.1 Introduction

In the developing world, labor force participation for women is low at 52 percent–about 26 percentage points lower than it is for men (Duflo 2012, World Development Report 2012). Industrialization and globalization are expanding economic opportunities for women, creating jobs in the manufacturing and service sectors that yield higher returns than the traditional agricultural sector. Access to such opportunities has encouraged women to invest in human capital and enter the formal labor market (Munshi and Rosenzweig 2006, Atkin 2009, Heath and Mubarak 2012, Jensen 2012). However, little is known about how women are affected by working in these newer sectors. In this paper, I explore the effects of working for longer periods in the manufacturing sector on women's marriage and fertility outcomes in rural India.

Empirically, identifying the causal effect of time spent in the formal labor force presents several challenges. Most of the existing literature exploits variation in access to employment to study the impact of labor market opportunities for women (Atkin 2009, Heath and Mubarak 2012, Jensen 2012). While this sheds light on the extensive margin, women who work outside the household may differ along other characteristics such as how liberal their families are, or the outside options and opportunity costs available to them. These differences can have a direct effect on later-life outcomes.

This paper considers the intensive margin and isolates the effect of duration worked on outcomes for women who took up the same kind of employment. Specifically, my analysis exploits a natural experiment created by a large Indian textile firm's decision to change fixed-term contracts to daily wage contracts. The firm's decision led to variation in the duration of employment for different workers. Administrative data shows that unanticipated differences in duration of exposure to the fixed-term contract affected the length of employment (in months). I survey all cohorts of workers affected by this change in contract, tracking them 4.5 years (on average) after they first started working at the firm. Using survey data for 985 women, I find that being employed longer increases the age of marriage and lowers desired fertility, without any observable costs on the marriage market and eventual spouse quality. There are also strong spillover effects within the workers' families such as an increase in the age of marriage for younger sisters and a decrease in school dropout rates for younger brothers. I find evidence in support of the hypothesis that an increase in empowerment and autonomy is a plausible channel for these effects.

Leaving the village before marriage is uniquely associated with the recent growth of employment in the manufacturing and service sectors. In traditionally conservative societies, participating in the formal labor market can lead to a particularly dramatic change in life exposure for women. It provides women with opportunities to gain different skills, earn

independent incomes and develop new social networks. The effect of this exposure on their ultimate economic and social outcomes, however, is ambiguous. On the one hand, it may increase women's bargaining power, thereby leading to better later-life outcomes. On the other hand, women who leave their native villages for employment may be looked upon unfavorably which could adversely impact their self-esteem and marriage outcomes.

The textile industry in the Indian state of Tamil Nadu offers a unique setting to study this question. In this industry, it is common for women to migrate from their parental villages to the location of the firm. They live and work at the factories with other women. They are often employed using fixed-term contracts with a large deferred payment that is given only upon completion of the duration specified by the contract. These contracts provide a strong incentive for tenure.

The firm I study replaced fixed-term contracts of three- and one-year lengths with contracts that paid workers a daily wage with no deferred payment. The change in the type of contracts was unanticipated by the workers. The new contract removed the tenure incentives previously in place for the workers. The workers from different cohorts were exposed to the fixed-term contract for different lengths of time before the change. I restrict the analysis to the sample of workers that joined before the change, and thus all selected into the fixed-term contract. These workers only differ from each other in the number of months worked for the firm before the change in contract occurred. Specifically, I use the duration of exposure to the three-year contract as an instrumental variable (IV) for the duration the woman works outside the village. Further, to control for any time trends associated with the cohort of joining the firm, I use the workers with one-year contracts as a control group. The difference-in-differences estimates for the first-stage highlight that for every month of exposure to the three-year contract, duration worked increases by 0.5 months.

Most women take up this type of employment in the window between their schooling and marriage. Therefore, work tenure may immediately affect marriage outcomes. Women who work for longer periods may marry later than those who work for a shorter duration. I find strong evidence that employment outside the village increases the age of marriage and decreases the probability that a woman is married by age 21. The IV results suggest that the elasticity of age of marriage with respect to duration worked is 1.1. While this is only slightly more than a one-for-one increase, it does not appear that completing the employment spell and getting married occur simultaneously. Instead, I find that women who work for longer periods receive their first marriage proposal at a later age. They also have a longer gap between receiving their first marriage proposal and getting married. This suggests that delays in marriage may occur partly because women who work longer choose to defer marriage even after receiving a proposal.

Early marriage for women is associated with a number of poor outcomes such as lower economic and social status (Dahl 2010). Work from Bangladesh suggests that delaying marriage increases use of preventive healthcare by women (Ambrus and Field 2008). In the setting in this paper, working may improve a woman's marriage outcome by changing her outside option and the pool of eligible spouses. However, there may also be potential costs to working and delaying marriage. If living and working outside the village is not desirable behavior in the marriage market, these women might find it harder to find a spouse and may end up not getting married at all. They could also be matched to spouses of lower quality, and forced to pay a larger dowry to compensate for having worked and being older at the time of marriage. However, the analysis shows that there are no significant effects on the number of marriage proposals received, the likelihood of being married, the dowry the woman's family has to give the spouse during the wedding and the eventual quality of her spouse.

Age of marriage has also been shown to significantly predict the age of first pregnancy and total fertility rate (Jensen and Thornton 2003). I find evidence that working for a longer period increases the age at which a woman has her first child and decreases the number of children the woman has had at the time of the survey. However these results should be interpreted cautiously since I only observe short- to medium-term outcomes for these women, and most women in the sample have not yet realized their life-time fertility. To address this concern, I examine the effect on desired life-time fertility, and find that for the average woman in the sample who works 18 months, desired fertility decreases by 14 percent.

Finally, I look at the impact of a woman being employed outside the household on her family members, and particularly on younger siblings. Younger siblings could be directly affected if they also enter the labor market following the sister, or indirectly impacted through spillovers from the older sister. I find that an older sister being employed does not increase the likelihood of her younger sister working, but does increase the age of marriage for her younger sister. Further, for younger brothers, an older sister working reduces school dropout rates and the likelihood that they have entered the labor market. These results suggest that there may be positive externalities for younger siblings when women work outside the home and that these externalities may apply even when the siblings themselves do not work.

I consider two possible channels through which the above mentioned effects could be taking place. First, working can increase female empowerment through the exposure to life outside the village, formation of new networks at the workplace, change in the worker's outside option and opportunity to earn an independent income. This, in turn, can change bargaining power and translate into changes in the real outcomes we observe

such as marriage and fertility. Additionally, women who work longer may have contributed more to their households' overall wealth which may result in delaying marriage and lower fertility for girls, and lower school dropout rates for boys.

To shed light on these channels, I measure the impact of duration of work on intermediate outcomes that measure empowerment and household wealth. Supporting the empowerment channel, I find that women who have worked longer score higher on measures of empowerment and autonomy. Particularly notable is the impact of working on autonomy in marriage decisions. In India, where a large number of women have arranged marriages and meet their spouse for the first time on their wedding day, this represents significant progress with respect to female empowerment (Banerji, Martin and Desai 2008). In contrast, I do not find conclusive evidence that an increase in household wealth is the primary channel for these effects.

The results in this paper are relevant to the literature on the impact of labor force participation on women in developing countries. Access to employment in the service sector for women in rural areas has been found to reduce early marriage and desired fertility by encouraging them to enter the labor force or obtain more education and training (Jensen 2012). The growth of manufacturing jobs has been associated with improvements in girl's school enrollment and better health for female children driven by increased returns to investment in them (Heath and Mubarak 2012, Atkin 2009). But these studies do not examine the impact of exposure to such work on the employed women themselves. Evidence from the textile industry in Bangladesh associates working with higher female status and better quality of life measures (Kabeer 2002, Hewett and Amin 2000). However, NGOs and human rights groups frequently highlight the negative effects of factory work on women such as long hours, exploitative and unsafe working conditions and social taboos (ActionAid, New York Times). This paper provides empirical evidence on the

effects of duration of work on later-life outcomes for the employed women.

This paper also contributes to the literature on policies that affect marriage and fertility. Compulsory schooling laws and lowering the costs of schooling can delay marriage by keeping girls in school for longer (Kirdar, Tayful and Koc 2011, Duflo, Dupas and Kremer 2011). However, with policies that affect marriage through schooling, women continue to reside at home without living independently outside the village, and this may not have the same impact on female autonomy. Goldin and Katz (2000, 2002) and Bailey (2006) find that the oral contraceptive pill led to delayed marriage and pregnancy and lower desired fertility in the United States by decreasing the cost of delaying marriage and allowing women to invest in careers. The setting in the paper provides evidence that opportunities that bring women in traditionally conservative societies outside their villages for employment could act as important tools for increasing female empowerment, and impact real outcomes for both the woman and her younger siblings.

Social norms and cultural beliefs related to gender roles and attitudes may be hard to change. However, living and working outside the village is not the only way through which gender attitudes and outcomes can change. Existing literature has studied the impact of mobility and exposure to life outside their community for women through other channels. For example, Jensen and Oster (2009) find that the introduction of cable television has significant impact on gender attitudes in rural India, which also translates into increased schooling for women and a decrease in fertility. They argue that this is because television portrays life in urban settings and dramatically changes the information available to these women. Beaman et al. (2012) use a natural experiment that reserves leadership positions for women in village councils. They show that female leadership influences adolescent girls' career aspirations and educational attainment. This paper contributes to this literature by examining the link between young women working

outside the village and their empowerment. The results show that longer duration of employment can increase female empowerment.

The rest of this paper is organized as follows. Section 1.2 describes employment patterns for women in the textile industry. Section 1.3 discusses the change in wage policy that provides the setting for a natural experiment in duration of work, explains the identification strategy and describes the data. Section 1.4 presents the main empirical results on marriage, fertility and spillovers to siblings. Section 1.5 explores possible mechanisms for the findings. Section 1.6 concludes by discussing the implications of the findings for policy and highlights avenues for further research.

1.2 Background on Working in Textile Industry

1.2.1 Textile Industry in Tamil Nadu

The opportunities for young women to work in the formal labor market are particularly salient for South Asia. The recent decades have seen a surge in labor market opportunities in this region, where industries have been rapidly growing, hence, creating opportunities for women that did not previously exist due to social, cultural, and economic reasons. The textile industry is one of the largest manufacturing sectors in South Asia that employs women. In India, textiles are a major contributor to industrial production and exports, accounting for nearly 14 percent of the region's total industrial production and 17 percent of its total export earnings (Gera 2012). Over the last two decades, the proportion of young women employed in the textile industry has increased since they are easy to manage, can be paid lower wages and are less likely to unionize than men (Standing 1999, Fontana 2003). The South Indian state of Tamil Nadu employs over 200,000 women in low-skill manufacturing jobs in this industry.

Textile firms in Tamil Nadu often hire young unmarried women under employment contracts that provide strong incentives for work tenure. Under these contracts, the firms hire women for three-year periods during which the women live and work at the textile factory. These factories provide dormitories, food and other facilities for the workers. During the contract period, the firms defer approximately one third of the workers' monthly wages. At the end of the contract period, the firm gives the workers the accrued wages as a lump sum of money. If the worker leaves before the end of the contract, she forfeits the entire lump sum of money. The lump sum thus provides a strong incentive to stay at the factory for the complete duration of the contract.

The women hired under such wage contracts are typically unmarried, since married women are less likely to move from their spouses' household to live in factory dormitories. They are around the ages of 17 or 18 years, have discontinued their schooling and come from low-income families in rural areas where the monthly household income is less than $100 (Neetha 2001). Despite the lock-in period of three years, families may find the contracts attractive in an environment where they face short-term credit constraints and uncertainty surrounding the timing of marriage. With limited opportunities for young women to work, employment in textile firms offers families an additional source of monthly income. It provides young women with a relatively secure living environment away from home. Further, the deferred lump sum payment may allow families to save large sums of money. In a society where a woman's wedding is a large expense on her family, this saving can be used to buy jewelry and pay for other wedding-related expenses. In fact, when they were first introduced, these employment contracts were often advertised as a way for women to save for their weddings.

1.2.2 Young Women Working Outside the Village

Many women in rural India seldom leave their village before marriage; for example, in India, while 75 percent of women aged 22 and older reside outside their place of birth, 87 percent of them do because of marriage migration (Fulford 2013). Moreover, in keeping with tradition, women in these regions marry very young, at about 20 years old (Das and Dey, 1998). If women work before marriage, they are typically engaged in agricultural work. Therefore, typically many women move directly from their parent's homes to their spouse's, having never lived independently and with no exposure to life outside their community.

Leaving the village for employment is a fairly new phenomenon that has occurred as a product of industrialization and globalization. Living and working outside the village for the first time can have a very significant impact on young women. First, these women are exposed to life outside their communities. They interact with other workers and management who may come from different places, and may learn more about life in different communities. Second, they live away from their families and may have the opportunity to negotiate independent decisions. Third, they live in a close setting with other young women in their age group and form new friendships and networks with these women which they may continue to maintain even after they stop working. Finally, the work experience may increase their future employability, changing their outside option. These effects could increase empowerment, autonomy and bargaining power for women.

Historically, factory employment where women live and work in the place of employment is not uncommon. In fact, it has been associated with gains to female autonomy and empowerment. The setting is similar to the employment of female workers in textile mills in Lowell in nineteenth century United States and women in Japan and China in

the early twentieth century (Dublin 1979, Dublin 1981, Eisler 1977, Honig 1996). The "Lowell Mill Girls" were the first generation of female workers during the Industrial Revolution in the United States. These women worked at the mills and attained economic independence for the first time. Eventually, when factory work became oppressive, these women protested and formed the first union of working women in the United States. Thus, despite the criticism of factory work by NGOs and human rights groups for exposing women to potentially exploitative and unsafe conditions, female empowerment from such opportunities may still lead to some positive outcomes.

1.3 Methodology

1.3.1 Natural Experiment: Change in Wage Policy

The firm I study operates several textile units in different parts of Tamil Nadu. I focus on the changes implemented at two particular units. At these units, the firm offered two types of contracts as of 2005. It offered a three-year contract to workers who entered the firm with no previous experience and one-year contract to workers who enter the firm with some previous experience. Under both contracts, the firm deferred approximately one third of the wage payment until the end of the fixed term. In 2010, following a change in its ownership structure, the firm terminated both contracts and switched to paying workers regular wages based on a daily wage and the number of days worked per month.[1] The main change in the wage structure involved workers receiving their entire monthly wages (no wages were deferred). The new contract removed incentives for workers to stay with the firm for longer periods of time. The change came into effect at once and the daily wage went up sufficiently to compensate for the amount deferred

[1]Detailed interviews with the management and owners suggest that the change was unrelated to the firm or unit profitability. They suggest that the workers could not anticipate the contract change. This is also confirmed through several focus groups with the workers who were at the firm during the change.

under the original contract. Moreover, workers were also given a settlement amount proportionate to the duration they had already worked under the fixed-term contract up to that point to compensate them for the change in the system. There were no other major changes to the work environment at this time. I use the change in wage contract by the firm as a natural experiment that affected the duration worked under the fixed-term contract.

Under the fixed-term contracts, a portion of the wage was deferred until the end of the term specified by the contract at which point it was given as a lump sum. If the worker failed to complete the contract period, she forfeited the deferred amount. This feature of the contract provided a strong incentive for the workers to complete the duration specified by the contract. The longer the time already spent at the firm, the higher the cost of quitting without completing the contract period. Once the deferred payments feature of the contract was removed, the workers did not face any cost to quitting since they received their full wages each month without any deferred amount. Thus, under the fixed-term contract, we expect to see weakly longer tenure at the firm relative to the daily wage contract. Moreover, depending on when the worker joined the firm, she would have been exposed to the fixed-term contract for a different period of time. We thus expect that women who have been exposed to the fixed-term contract for a longer period also work for the firm for a correspondingly longer period of time.

Figure 1.1 uses administrative data to plot the Kaplan-Meier survival estimates for three-year contract workers from different cohorts before and after the policy change. Cohorts are defined based on when the worker joined the firm. The 2006 cohort was fully exposed to the original fixed-term contract. The survival estimates for this cohort show a gradual decline initially followed by a flat region until the end of the contract period after which there is a steep drop. On the other hand, for the cohort that joined

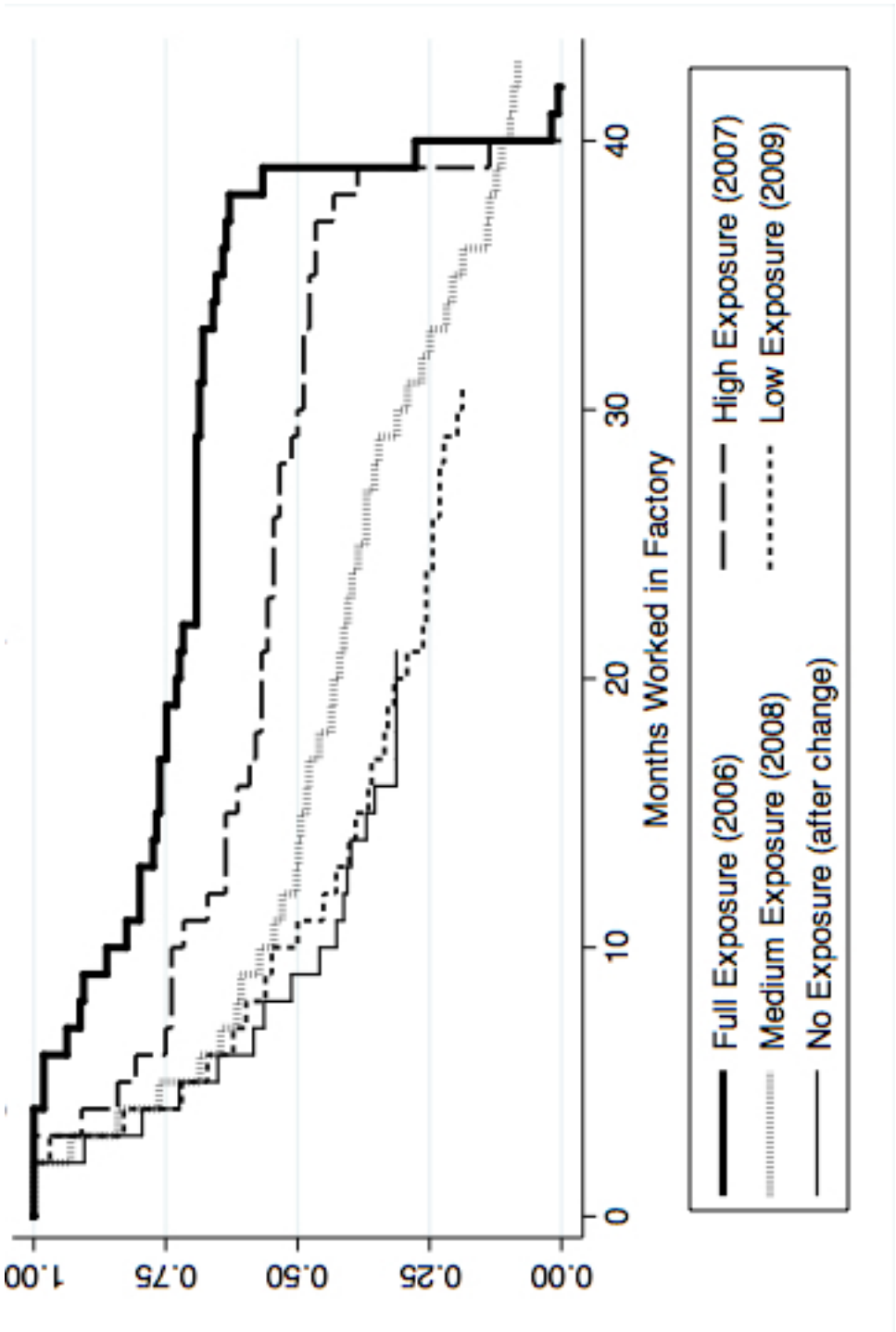

Figure 1.1: 3 *Year Contract: Kaplan-Meier Survival Estimates*

after the change in wage policy such that they were never exposed to the fixed-term contract, the plot of the survival estimates shows a steady and gradual decline with overall lower duration of work. For the cohorts in between (2007, 2008, 2009) that were exposed to the fixed-term contract for different durations, we see a gradual change in shape of the survival estimates from that of the 2006 cohort to that of the cohort after the policy change. This shows that average duration of work increased with exposure to the fixed-term contract where exposure is defined as the number of months before the change in wage contracts the worker joined the firm. Exposure to the fixed-term contract is a good predictor of duration worked at the firm and offers a valid instrumental variable.

1.3.2 Identification Strategy

The purpose of this study is to identify the causal impact of duration worked on later-life outcomes. The simple OLS estimation is given by:

$$y_{ic} = \beta_0 + \beta_1 W_{ic} + \beta_2 3^{yr} C_{ic} + \beta_3 X_{ic} + \gamma_c + \epsilon_{ic} \tag{1.1}$$

where y_{ic} is the outcome of interest for worker i from cohort of joining c, W_{ic} is the number of months worked at the factory, $3^{yr}C_{ic}$ is a dummy for whether the worker had a three-year contract at the start of the employment and X_{ic} is a set of individual characteristics such as age and education. γ_c is a set of cohort of joining fixed effects for each six-month cohort before the policy change. However, the coefficient β_1 may be biased and, hence, may not give the causal impact of duration of employment since duration worked may be endogenous to the outcomes of interest. For example, a worker from a more traditional family may tend to work less and get married early.

To identify the causal effect of duration of employment, I require quasi-exogenous variation in the duration worked. The sudden termination of the fixed-term contract by the firm provides an exogenous shock to highly incentivized work tenure for the workers.

Workers who joined the firm prior to the change were exposed to the fixed-term contract for different periods of time based on when they joined the firm relative to the policy change. For example, a worker who joined the firm the month before the change spent only a month under the fixed-term contract compared to a worker who joined three years before the change and had almost completed her three-year term as specified by the original contract.

I define the exposure to the fixed-term contract as the number of months before the change in wage contract a worker joined the firm. This allows for the possibility that some workers may have left the firm of their own accord before completing the term unrelated to the contract change since I only consider how many months before the change they joined. I use this exposure variable as a source of variation for duration worked and use an instrumental variable (IV) approach to measure the causal impact of working on life outcomes.

There are two main concerns with this identification strategy. First, very old and recent cohorts of workers may be different from each other. In particular, the workers who joined after the change in policy selected into a different contract and hence may be different on other dimensions. To account for this selection bias, I restrict the analysis to workers who were hired before the wage contract was changed. Within this sample, all the workers had originally selected into the same contract, and hence there is no selection bias due to contract choice. Further, I drop very old cohorts and restrict the analysis to consider only the workers falling in between three years before the contract change and those hired right before the change (2007 to 2010). Within this sample, I use all workers even if they were no longer working at the firm when the change came into effect. For example, in earlier cohorts there may be workers who dropped in the first few months despite the fixed-term contracts, and these workers may differ on other dimensions that affect the

outcomes of interest. Not including these workers would bias the analysis by dropping out non-compliers from earlier cohorts and differentially changing the composition of the cohorts.

Second, the identification strategy relies on the fact that the policy change affects women to different extents depending on when they joined the firm. The duration of exposure to the fixed-term contract depends on which cohort the worker joined and is confounded by a possible time trend and other differences related to cohort of joining. To address this concern, I use all workers at the firm who were hired on a one-year contract between 2007 and 2010 as a control group. I also include cohort of joining fixed-effects. The most recent cohort of one-year contract workers in my sample are actually affected by the change in policy since they were switched from a one-year contract to a daily wage contract. However, to be conservative in controlling for cohort of joining effects, I use these workers only as a control group to difference out cohort of joining effects.

Figure 1.2 shows the first stage by fitting a fractional polynomial from the data. It plots the number of months worked at the firm against the hiring date, i.e. the number of months before the change in wage contract the worker joined the firm. There is a strong effect of exposure to the fixed-term contract for workers with the 3-year contract, but none for workers on the one-year contract.

As a first stage in my analysis, I estimate the differences in difference of duration worked between three-year and one-year contract workers from different cohorts controlling for observable individual characteristics such as age and years of education.[2] I use the following specification for the first stage:

[2]Figure A1 in the appendix which plots the distribution of age in the sample by year of joining shows that there is variation in age across cohorts.

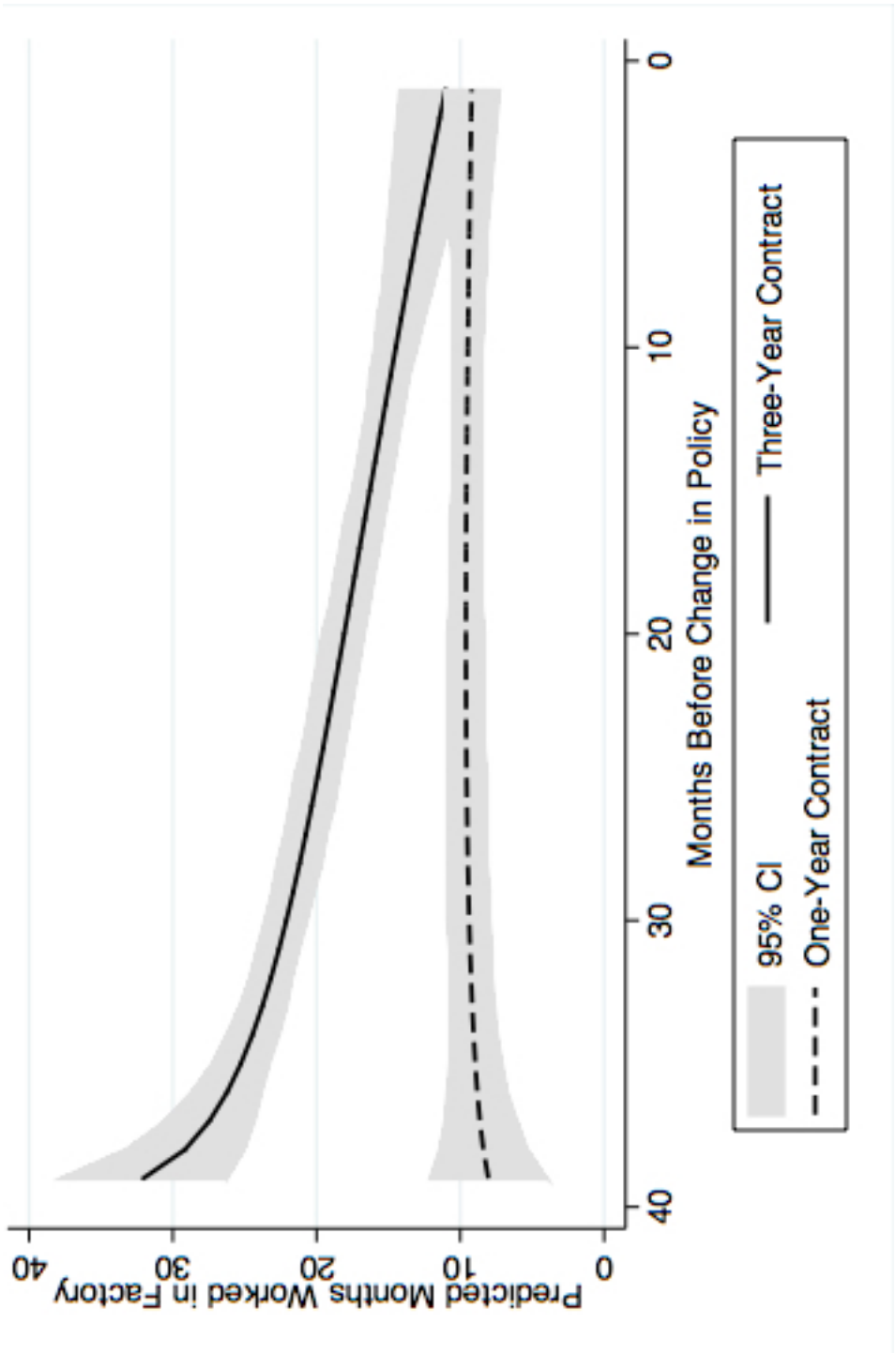

Figure 1.2: *First Stage: Diff-in-Diff of Months Worked Outside*

$$W_{ic} = \alpha_0 + \alpha_1 E_{ic} * 3^{yr}C_{ic} + \alpha_2 E_{ic} + \alpha_3 3^{yr}C_{ic} + \alpha_4 X_{ic} + \gamma_c + \epsilon_{ic} \tag{1.2}$$

where E_{ic} is the exposure to the fixed-term contract defined as the number of months before the change in wage policy the worker i from cohort c joined the firm. α_1 is the difference in differences estimate that measures the difference in duration worked between three- and one-year contract workers from different cohorts.

I then estimate the causal effect of working on marriage, empowerment and the other outcomes of interest using the interaction between E_{ic} and $3^{yr}C_{ic}$ as an instrumental variable for W_{ic}. The variable E_{ic} is unrelated to the outcomes since the change in wage policy by the firm was an exogenous shock and satisfies the exclusion restriction. The IV approach involves estimating the following two-stage model:

$$y_{ic} = \beta_0 + \beta_1 W_{ic} + \beta_2 E_{ic} + \beta_3 3^{yr}C_{ic} + \beta_4 X_{ic} + \gamma_c + \epsilon_{ic} \tag{1.3}$$

$$W_{ic} = \beta_0^{'} + \beta_1^{'} E_{ic} * 3^{yr}C_{ic} + \beta_2^{'} E_{ic} + \beta_3^{'} 3^{yr}C_i + \beta_4^{'} X_{ic} + \gamma_c^{'} + \mu_{ic} \tag{1.4}$$

where y_{ic} is the outcome of interest for worker i from cohort c. β_1 estimates the causal effect of working one additional month in the factory for women who selected into the three-year contract.

The reduced form gives the effect of exposure to fixed-term contracts as follows:

$$y_{ic} = \alpha_0 + \alpha_1 E_{ic} * 3^{yr}C_{ic} + \alpha_2 E_{ic} + \alpha_3 3^{yr}C_{ic} + \alpha_4 X_{ic} + \gamma_c + \epsilon_{ic} \tag{1.5}$$

The IV approach allows me to isolate the causal effect of working on life outcomes for all women who took up this employment.

1.3.3 Survey and Data

I use two sets of data for the analysis. First, I collected administrative data with employee records from the firm. These records provide a complete list of all female workers hired starting in 2005, basic demographic information, contact information provided at the start of the employment period and the dates of starting and completion employment at the firm. The records also note if the worker was under the three-year or the one-year contract.

The firm's data allowed me to select the sample for a socio-economic survey with a focus on measuring marriage and empowerment outcomes. For the survey, I selected all workers hired from 2007 until the implementation of the wage policy change. I also restricted my target sample to workers who worked for at least one month at the firm, leaving me with a sample of 1414 workers. Of these workers, 616 workers were working at the firm at the time of the wage policy change. The follow-up survey was complicated due to the fact that most workers had initially migrated from different districts within Tamil Nadu and many no longer worked at the firm. I thus designed, piloted and implemented a multi-step tracking process to identify the location of the workers (or family members) for the survey, to ensure minimal attrition from the sample. The tracking process was able to successfully track and complete surveys for about 70 percent of the sample. I describe the different stages of the process used to track respondents in Section A1 in the appendix.

Table A2 in the appendix shows the tracking results by cohort of joining. In constructing the instrumental variable, I use the fact that workers from different joining cohorts were affected differentially by the policy change. I consider two measures of tracking success - whether the survey was successfully completed and whether the worker was tracked but refused the survey. I regress these variables on the dummies for the cohort in which the worker joined the firm and a dummy for whether the worker had a three-year contract. I find that the probability of completing the survey successfully is about 15 percent lower

for the cohort that joined the firm 24 to 30 months before the change in wage policy by the firm. Hence, as a robustness check, in all my subsequent analysis I examine the effects for the restricted sample which omits this cohort to reduce any bias that may come from the lower tracking rate for workers in this cohort.

Since the tracking and survey process only captured about 70 percent of the original sample, differential attrition by exposure to the fixed-term contract may present a concern. I thus test whether exposure to the fixed-term contract has an effect on the probability that the survey was completed and the probability that the worker was tracked but refused to participate in the survey using the reduced form specification. The results presented in Table A3 in the appendix show that there are no significant differences in attrition by duration of exposure to the fixed-term contract. A second fact to note is that some questions and sections in the survey were added after the pilot round was completed. Moreover, in cases where the worker was unavailable and a family member was surveyed, I restricted the questions to those measuring real outcomes. Hence for such surveys I do not have all the outcomes. However, there are no differences by exposure to the fixed-term contract on whether the worker was in the pilot round or that a family member was surveyed.

Table 1.1 presents summary statistics for the surveyed sample. It also shows the balance checks for observable individual characteristics. Columns (1) and (2) show summary statistics for the three-year contract and one-year contract workers respectively. About two thirds of the sample was employed under the three-year contract (664 workers) and the remaining third has the one-year contract (321 workers). On average, the workers with three-year contracts are younger; the average current age of workers with the three-year contract is about 22 years while that of workers with the one-year contract is about 24 years. This is consistent with the fact that workers with the one-year contract were given shorter contracts because they had previous work experience and hence we

Table 1.1: *Summary Statistics and Balance of Individual Characteristics*

	3 Year Contract (1)	1 Year Contract (2)	Full Sample (3)	Restricted Sample (4)
Age	22.06	24.04	-0.03	-0.03
	(2.29)	(2.78)	(0.02)	(0.02)
Years of Education	8.93	8.82	-0.02	-0.01
	(1.92)	(1.82)	(0.01)	(0.01)
Natal Household Size	5.09	5.16	0.00	0.01
	(1.47)	(1.60)	(0.01)	(0.01)
Number of Siblings	2.44	2.51	-0.00	-0.00
	(1.40)	(1.43)	(0.01)	(0.01)
Birth Order	1.16	1.42	-0.01	-0.01
	(1.31)	(1.41)	(0.01)	(0.01)
Natal Family Lives in District Around	0.30	0.38	-0.00	-0.00
Factory	(0.46)	(0.49)	(0.00)	(0.00)
Widowed Parent	0.16	0.24	-0.00	-0.00
	(0.36)	(0.43)	(0.00)	(0.00)
Father's Occupation: Agric Labor	0.47	0.43	-0.00	0.00
	(0.50)	(0.50)	(0.00)	(0.00)
Mother's Occupation: Agric Labor	0.53	0.47	-0.00	-0.00
	(0.50)	(0.50)	(0.00)	(0.00)
Mother: Housewife	0.18	0.21	0.00	0.00*
	(0.38)	(0.21)	(0.00)	(0.00)
Sibling Worked in a Manufacturing	0.18	0.22	-0.00	0.00
Job with Worker	(0.39)	(0.41)	(0.00)	(0.00)
Observations	664	321	985	911
Individual Controls	-	-	No	No
Cohort of Joining Controls	-	-	Yes	Yes

Notes:

(1) In columns (1) & (2) means for 3 year and 1 year contract workers are reported.

(2) In columns (3) & (4) the coefficient on the interaction between the duration of exposure and the dummy for the three-year contract is reported for full sample and for restricted sample excluding the cohort that joined 24 to 30 months before the wage policy change.

(3) In columns (1) & (2) standard deviations in parenthesis and in columns (3) & (4) robust standard errors in parentheses

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

expect them to be older. The workers with the three-year and one-year contracts both have about 9 years of education, come from families with approximately 5 members and on average have a similar number of siblings. The workers with a one-year contract have a slightly higher birth-order than those with a three-year contract. They are also a little more likely to be from a district around the factory and to come from a household with a widowed parent.

Regardless of contract type, most workers come from agricultural families. For three-year contract workers the father's primary occupation is about 47 percent likely to be agricultural labor while this is about 43 percent for the one-year contract workers. The mother's primary occupation is agricultural labor for about 53 percent of three-year contract workers and 47 percent of one-year contract workers. For 18 percent of three-year and 21 percent of one-year contract workers, the mother is a housewife. For a small fraction, about 18 percent of the three-year contract workers and about 22 percent of the one-year contract workers, a sibling has worked at a manufacturing job with the worker.

Columns (3) and (4) show balance checks for differences in worker characteristics by duration of exposure to the fixed-term contract. I use the reduced form specification without individual controls and report the coefficient on the interaction term in columns (3) and (4) for the full and restricted samples respectively. Workers who were exposed to the three-year contract for longer are younger and have fewer years of education, but this is not significant at the 10 percent level. However, I include age and education controls in all the main specifications to account for any potential differences in outcomes by age or education. I find no significant differences by length of exposure to the fixed-term contract on any of the other observable characteristics.

1.4 Main Results

This section presents the difference in differences results for the first stage and the IV and reduced-form results for the impact of working on marriage, fertility and spillovers to siblings. I show the IV and reduced form results for the full sample from the survey. Results for the restricted sample omitting the cohort of workers who joined the firm 24 to 30 months before the change in wage contract are provided in Section A3 in the appendix.

1.4.1 Impact of Fixed-Term Contract on Duration Worked

Columns (1) and (2) of Table 1.2 show the simple difference for duration worked in the factory by exposure to the fixed-term contract separately for the three- and one-year contract workers respectively. The effect of exposure to the fixed-term contract is large and significant for the three-year contract workers but is smaller and not statistically significant for the one-year contract workers. Columns (3) to (6) of Table 1.2 show the difference in differences results for the impact of exposure to the fixed-term contracts on duration worked. For every month of exposure to the fixed-term contract, duration worked by three-year contract workers increases by 0.5 months. Columns (3) and (4) show the results for the full and restricted samples using administrative data from the firm on employment spell lengths.

One concern with the above estimates is that the duration worked at the firm might not be a good measure of the total duration of employment outside the home. Workers may work at other similar jobs in other firms once the wage policy change occurs. In columns (5) and (6), I use a variable from the occupation history collected during the survey that measures total duration worked across all jobs. This includes any time spent working in agricultural jobs. I find that the results for the impact of exposure to the fixed-term contract on duration worked continue to hold suggesting that it is not just the duration

Table 1.2: *First Stage: Effect of Original Wage Contract on Duration Worked*

	Months worked in factory		Months worked in factory		Total Work	
	(1)	(2)	(3)	(4)	(5)	(6)
Months before X 3 year			0.463***	0.456***	0.531***	0.556***
			(0.0660)	(0.0685)	(0.155)	(0.161)
			[0.000]	[0.000]	[0.001]	[0.001]
Months before policy change	0.874***	0.123	0.311**	0.278*	-0.366	-0.531*
	(0.207)	(0.138)	(0.135)	(0.155)	(0.278)	(0.317)
	[0.000]	[0.375]	[0.021]	[0.074]	[0.188]	[0.094]
Sample	3 year	1 year	Full	Restricted	Full	Restricted
Observations	664	321	985	911	776	719

Notes:
(1) Columns (1) and (2) shows the effect of exposure to the fixed-term contract on duration worked.
(2) Columns (3) to (6) show the difference-in-difference results for the exposure to the fixed-term contract on duration worked.
(3) In columns (1) to (4) the dependent variable is duration worked in the factory from administrative data.
(4) In columns (5) and (6) the dependent variable is total work reported from survey data.
(5) Results are presented for the full and restricted sample.
(6) The restricted sample drops the cohort that had a poor tracking rate in the survey.
(7) Individual controls for age and cohort of joining controls included in all specifications.
(8) Results are consistent with dropping cohort of joining fixed effects.
(9) The sample size is lower in columns (5) and (6) because in some cases the workers could not provide the duration at each job in their occupation history.
(10) Robust standard errors in parentheses and *p*-values in brackets.
(11) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

worked at the firm that is affected, but total duration worked also increases with exposure to the fixed-term contract.[3]

In summary, the results from the first stage indicate that the change in wage contracts by the firm had a large impact on the number of months worked at the firm. Workers who were exposed to the fixed-term contract got a larger settlement amount and we might expect the income effect from the liquidity shock to reduce labor supply.[4] However, the tenure incentives in the fixed-term wage contract had a stronger effect on duration worked and labor supply by the women. The first-stage results show that the interaction variable between exposure to the fixed-term contract and the dummy for the three-year contract is a good predictor of duration worked.

1.4.2 Impact of Working on Marriage and Fertility

Table 1.3 shows the IV and reduced form results for the effect of working on timing of marriage. For every month worked, probability of being married by the age of 21 reduces by about .01 (Column (1)). This translates to a decrease in the probability of being married before age 21 of more than 17 percent for the average worker in the sample with 18 months of work. It captures both whether the woman is married and the age at which she was married. I therefore also look at the intensive margin for the women who are already married. Here I find that for each additional month worked, the age of marriage increases by about 1.1 months, slightly more than a one-for-one increase (Column (2) of Table 1.3). This estimate is larger than other estimates found in the literature. A delay in timing of menarche has a smaller effect, with a one year delay leading to an increase in

[3]The total work variable is noisy since workers were sometimes unable to report exactly how long they worked at each of their other jobs. Therefore, in subsequent regressions for the IV approach, I use the variable measuring duration worked at the firm since this is measured accurately from administrative data.

[4]In results available on request, I find that a longer duration of exposure to the fixed-term contract and, hence, a larger settlement amount has no effect on duration worked after the change in wage contract.

Table 1.3: *Age of Marriage*

	Married before age 21 (1)	Age of Marriage (2)	Age received first proposal (3)	Time between first proposal & marriage (4)
Panel A: IV Results				
Months worked in factory	-0.00960*	0.0883**	0.0512*	0.0486
	(0.00532)	(0.0397)	(0.0268)	(0.0374)
	[0.071]	[0.026]	[0.056]	[0.193]
Panel B: Reduced Form Results				
Months before X 3 year	-0.00506*	0.0355**	0.0275*	0.0201
	(0.00285)	(0.0155)	(0.0145)	(0.0150)
	[0.076]	[0.022]	[0.058]	[0.181]
Sample Mean	0.318	20.61	19.91	1.059
Observations	948	595	833	505

Notes:
(1) Columns (1) to (4) of Panel A show the IV results for the impact of working outside the household on age of marriage.
(2) The change in wage policy by the firm is used as an instrumental variable for months worked in the factory.
(3) Columns (1) to (4) of Panel B show the reduced form results for the effect of duration under the old contract on age of marriage.
(4) Individual controls for age and education and cohort of joining fixed effects included
(5) Results are consistent with dropping cohort of joining fixed effects.
(6) About 40% of the sample is unmarried and hence the sample size in columns (1) and (4) is smaller.
(7) Robust standard errors in parentheses and *p*-values in brackets.
(8) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

age of marriage by 0.74 years (Ambrus and Field 2008). A higher age of menarche has a mechanical effect on marriage since women are typically withheld from the marriage market before puberty. The duration of employment may have a larger impact on the age of marriage if women who work longer also choose to defer marriage.

Figure 1.3 plots a distribution of the time between when a worker in the sample completes her employment spell at the factory and when she gets married. The plot suggests that there is no mechanical rule for this, i.e. it is not the case that women do not get married while at the factory, but then get married immediately after completing their employment and returning home. The results in columns (3) and (4) of Table 1.3 break down the delay into two components, the age the woman receives her first marriage proposal and the time between receiving this proposal and getting married to understand how the time to marriage is distributed. The results suggest that while some of the delay may be because women who work longer receive their first marriage proposal later, the time between receiving the first proposal and getting married also increases with working suggesting that these women may also be choosing to defer marriage or may be pickier because they have a better outside option.

While increasing the age of marriage for women is often considered an important policy goal in many developing countries, the overall effect on the marriage market may be negative if women who work longer and delay marriage are matched to a spouse of lower quality. The results in Table 1.4 show that in the equilibrium there are no negative effects of working on observable characteristics of marriage outcomes. In particular, there are no significant effects of working on the number of marriage proposals a woman receives and whether the woman is married. Further, there is also no significant effect on the value of gifts the bride's family gives the groom and his family at the time of marriage suggesting that women do not have to pay a larger dowry to compensate for

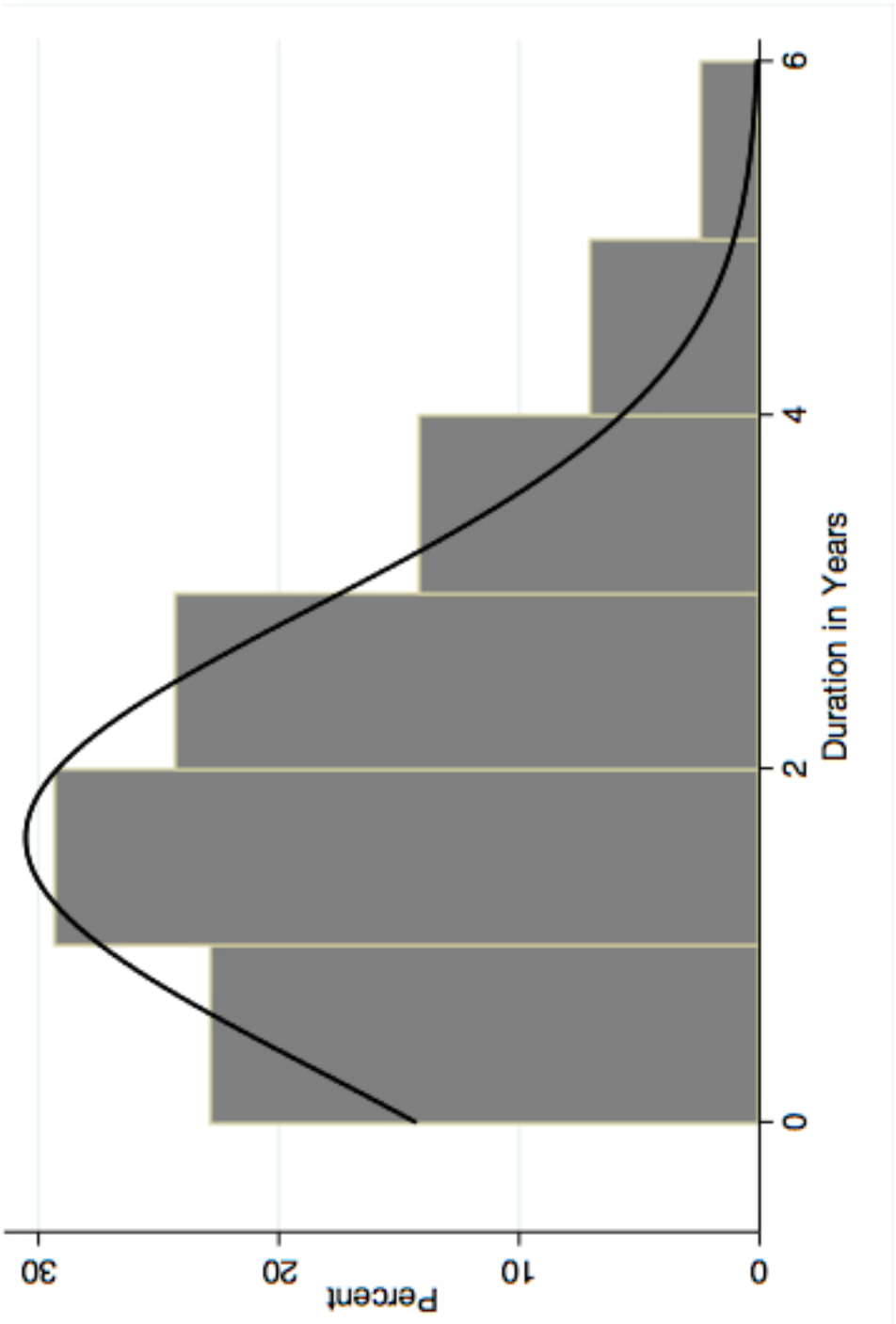

Figure 1.3: *Time Between End of Factory Work and Marriage*

Table 1.4: *Costs of Delaying Marriage*

	No. of marriage proposals received (1)	Ever Married (2)	Log Gifts given during Wedding (3)	Spouse Quality Index (4)
Panel A: IV Results				
Months worked in factory	-0.0294	-0.00227	-0.00465	-.0033
in factory	(0.0467)	(0.00569)	(0.0765)	(0.0096)
	[0.529]	[0.690]	[0.952]	[0.726]
Panel B: Reduced Form Results				
Months before X 3 year	-0.0155	-0.00110	-0.00181	-0.00126
	(0.0249)	(0.00281)	(0.0304)	(0.00391)
	[0.535]	[0.695]	[0.953]	[0.748]
Sample Mean	2.996	0.612	178240.4	.
Observations	847	981	585	.

Notes:
(1) Columns (1) to (3) of Panel A show the IV results for the impact of working outside the household on the marriage market.
(2) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(3) Columns (1) to (3) of Panel B show the reduced form results for the effect of duration under the old contract on the marriage market.
(4) Columns (4) shows the average effect size for the impact of working outside the household on spousal quality and the effects can be interpreted as standard deviation changes.
(5) Please refer to the appendix section 2 for the composition of the index.
(6) Individual controls for age and education and cohort of joining fixed effects included.
(7) Results are consistent with dropping cohort of joining fixed effects.
(8) About 40% of the sample is unmarried and hence the sample size in column (3) is smaller.
(9) Robust standard errors in parentheses and *p*-values in brackets.
(10) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

working and getting married at an older age.

I next consider an index which includes variables that measure various dimensions of spousal quality such as the age gap between the worker and her spouse, whether the spouse lives in a different village or district, the relative economic status of the spouse, the relative education of the spouse and the reported income of the spouse.[5] In column (4) I present the average effect size (AES) for the equilibrium quality of spouse. The method which follows O'Brien (1984), Kling et al. (2004) and Clingingsmith et al. (2009) computes the average effect size across outcomes as the average of the individual effects standardized by the standard deviation of the effect for the comparison group.[6] I find no significant effects of working on spouse quality in the equilibrium.[7] I can reject a decline in spouse quality of greater than 0.02 standard deviations with 95 percent confidence.

The results in Table 1.4 suggests that on observable dimensions, women do not suffer any costs in the marriage market from being employed. It is important to note that these results are the equilibrium outcomes in the marriage market. For example, spouse quality may worsen because the woman is older when she gets married, but may improve because of her work experience. The results reported here reflects the net effect of working for a longer time on the marriage market.

In Table 1.5, I examine the effect of working on age of first pregnancy and number of children. I find that the probability that the woman had a child before the age of 23

[5]Section A2 in the appendix provides details on the components of this index.

[6]To test for the AES against the null hypothesis of no average effect, the individual effects are jointly estimating in a seemingly unrelated regression framework. The stacked regression gives the correct covariance matrix for a test of the AES.

[7]In the results presented in Table A4 in the appendix, I show that there are no significant effects on any of the individual variables that make up the spouse quality index.

Table 1.5: *Fertility*

	Child before age 23 (1)	Age when first child was born (2)	No. of kids currently (if married) (3)	No. of kids currently (full sample) (4)	Desired Fertility (5)
Panel A: IV Results					
Months worked in factory	-0.0141**	0.0680	-0.0289*	-0.0272***	-0.0152**
	(0.00595)	(0.0513)	(0.0175)	(0.0100)	(0.00757)
	[0.018]	[0.186]	[0.098]	[0.007]	[0.044]
Panel B: Reduced Form Results					
Months before X 3 year	-0.00706**	0.0220	-0.0109	-0.0137***	-0.00796**
	(0.00304)	(0.0170)	(0.00692)	(0.00515)	(0.00380)
	[0.021]	[0.196]	[0.117]	[0.008]	[0.037]
Sample Mean	0.365	21.54	0.957	0.523	1.932
Observations	682	369	458	840	570

Notes:
(1) Columns (1) to (5) of Panel A show the IV results for the impact of working outside the household on fertility.
(2) The change in wage policy by the firm is used as an instrumental variable for months worked in the factory.
(3) Columns (1) to (5) of Panel B show the reduced form results for the effect of duration under the old contract on fertility.
(4) Individual controls for age and education and cohort of joining fixed effects included.
(5) Results are consistent with dropping cohort of joining fixed effects.
(6) About 40% of the sample is unmarried and in the pilot round of the survey we did not ask the number of children the woman had; therefore we do not have the full sample in columns (1) to (3).
(7) Desired fertility was added in a later version of the survey and hence has a smaller number of observations.
(8) Robust standard errors in parentheses and *p*-values in brackets.
(9) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

decreases by 0.01 for every month worked. This is more than a 25 percent decrease in the probability for the average worker in the sample (column (1)). Examining the intensive margin for the women who had a child at the time of the survey, in column (2), I find that the age of the woman when her first child was born increases, though the sample size is small and the results are not significant at the 10 percent level.

In columns (3) and (4), I present the results for the number of children for women who are married and for all women in the sample, respectively. I find that working for a longer period of time is associated with having fewer children. However, these results should be interpreted with caution due to the small sample size. Over 40 percent of the women in the sample remain unmarried and do not have children at the time of the survey. Moreover, I only observe fertility at the time of survey rather than fertility over the woman's entire lifetime and many of the women in the sample have not realized their lifetime fertility.

I therefore examine the effect of time spent working on desired fertility, i.e. the number of children the woman reports she would like to have. For the average worker in the sample, the results indicated a reduction in desired fertility by 0.27, a 14 percent decrease from the mean desired fertility of approximately 2 children in the sample (column (5)). This is fairly large and is comparable to the declines in desired fertility observed by Jensen (2012) in an RCT that offered women in rural India recruitment services for jobs in the BPO sector.[8]

[8]Jensen (2012) finds that desired fertility decreases by 0.35 from the control group mean of 3 children, almost a 12 percent decline.

1.4.3 Spillovers to Siblings

In this section, I examine whether a woman's employment status is associated with spillovers to her siblings.[9] I consider the impact of women working on their siblings' marriage, education and work outcomes. One could expect such effects on younger siblings because the woman may directly affect her siblings (due to changed attitudes), or because her work spell has changed the family's financial situation. In contrast, there should not be such effects for older siblings since most of these choices have been realized already. To examine this, I interact the duration worked with whether or not the sibling is an elder or younger brother or sister. I instrument this with the interactions of the instrumental variable with whether the sibling is an elder or younger brother or sister in this regression.

Table 1.6 provides the IV and reduced form results for the full sample. The age of marriage for younger sisters increases, and this increase is similar in magnitude to the increase in age of marriage for the worker. However, there is no effect on whether the younger sisters are currently studying or have ever worked which suggests that these increases are due to spillovers from the worker and not from the sister working herself. More empowered older sisters may expose their younger siblings to the new values they learn from working outside the village and bargain for better outcomes for their younger siblings. Alternatively, we may see this type of effect on age of marriage because in these societies female children are married by birth order and delaying the marriage of an older sister means her younger sisters will also get married later (Vogl 2013).

For younger brothers, I find an increase in the probability that they are currently studying and a decrease in the probability of having ever worked. Longer duration of employment for a woman may increase household wealth, which in turn may result in increased

[9]The pilot round of the survey did not include a roster of siblings and hence we do not have outcomes for siblings for surveys during the pilot stage.

Table 1.6: *Spillovers to Siblings*

	Ever Married (1)	Age of Marriage (2)	Currently Studying (3)	Ever Worked (4)
Panel A: IV Results				
Younger Sister X	-0.00447	0.0916*	-0.00125	-0.00872
Months Worked	(0.00668)	(0.0474)	(0.00717)	(0.00870)
	[0.504]	[0.053]	[0.862]	[0.316]
Younger Brother X	0.00429	0.144	0.0101*	-0.0105*
Months Worked	(0.00295)	(0.114)	(0.00571)	(0.00626)
	[0.146]	[0.207]	[0.078]	[0.094]
Older Sister X	0.000931	0.0199	0.000763	-0.00837
Months Worked	(0.00353)	(0.0487)	(0.00227)	(0.00817)
	[0.792]	[0.683]	[0.737]	[0.305]
Older Brother X	0.0000345	0.0241	0.00299	-0.00230
Months Worked	(0.00892)	(0.0784)	(0.00287)	(0.00429)
	[0.997]	[0.758]	[0.297]	[0.592]
Panel B: Reduced Form Results				
Younger Sister X	-0.00178	0.0428*	-0.000570	-0.00331
Months Before X	(0.00262)	(0.0222)	(0.00280)	(0.00328)
3 year contract	[0.498]	[0.055]	[0.839]	[0.313]
Younger Brother X	0.00195	0.0462	0.00453*	-0.00477*
Months Before X	(0.00127)	(0.0317)	(0.00258)	(0.00273)
3 year contract	[0.125]	[0.145]	[0.079]	[0.081]
Older Sister X	0.000402	0.00852	0.000298	-0.00386
Months Before X	(0.00165)	(0.0237)	(0.00109)	(0.00373)
3 year contract	[0.807]	[0.719]	[0.786]	[0.302]
Older Brother X	0.0000415	0.0112	0.00135	-0.00108
Months Before X	(0.00404)	(0.0407)	(0.00128)	(0.00190)
3 year contract	[0.992]	[0.783]	[0.292]	[0.568]
Observations	2467	1043	2467	2466

Notes:

(1) Columns (1) and (4) of Panel A and B show the IV and reduced form results for the impact of working on sibling's marriage, education and work.

(2) The change in wage policy by the firm is used as a instrument variable for months worked in factory.

(3) Includes individual sibling-level controls for age and worker-level controls for age, education, birth-order, number of siblings and number of younger brothers & sisters.

(4) Standard errors clustered by worker in parentheses and *p*-values in brackets

(5) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

education for siblings. If older girls' work is a substitute for younger boys' work, younger brothers may delay entering the labor market when their sisters work longer. Moreover, if sisters are getting married at a later age, this defers the family's wedding-related expenses. The deferred expenses may increase resources and allow younger boys to study longer and not enter the labor market early.

1.5 Mechanisms

The results in Section 1.4 show that working increases the age of marriage and lowers desired fertility without any observable costs in the marriage market. Moreover, when women work for a longer time period, there are spillovers to her younger siblings; younger sisters get married later and younger brothers delay entry into the labor market and remain in school. These changes could occur due to an increase in empowerment and autonomy for women or due to an increase in overall household wealth. In this section, I examine the effect of working on intermediate outcomes such as empowerment, autonomy and household wealth. I provide the IV and reduced form effects for the full sample. Again, the effects for the restricted sample are provided in Section A3 in the appendix.

1.5.1 Empowerment and Autonomy

In Table 1.7, I present the average effect sizes for different measures of empowerment and autonomy for the full and restricted sample.[10] Again, as with the spouse quality index in Table 1.4, I follow O'Brien (1984), Kling et al. (2004) and Clingingsmith et al. (2009) and present average effect sizes. Column (1) shows that duration of employment increases women's empowerment score which is based on responses to a series of questions that

[10]The pilot round of the survey did not include some of these questions. Moreover, in cases where we conducted a family survey we did not ask questions on attitudes and limited the questions to those on real outcomes. We do not have all the outcomes for empowerment and autonomy for those surveys

Table 1.7: *Empowerment and Autonomy*

	Empowerment	Internal Locus of Control	Marriage Decisions & Attitudes	Autonomy in Work Decisions
	(1)	(2)	(3)	(4)
Panel A: IV Results				
Months worked in factory	0.0079**	0.0109*	0.032***	0.0288***
	(0.0038)	(0.0058)	(.0099)	(0.0112)
	[0.036]	[0.060]	[0.001]	[0.010]
Panel B: Reduced Form Results				
Months before X 3 year	0.00464	0.00640*	0.0185***	0.0144***
	(0.00289)	(0.00366)	(0.00579)	(0.00544)
	[0.108]	[0.080]	[0.001]	[0.008]

Notes:
(1) Columns (1) to (4) show the average effect sizes for the impact of working outside the household on empowerment and autonomy and the effects can be interpreted as standard deviation changes.
(2) Please refer to the appendix section 2 for the composition of the index.
(3) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(4) Panel A and B show the IV and reduced form results respectively.
(5) Individual controls for age and education and cohort of joining fixed effects included.
(6) Results are consistent with dropping cohort of joining fixed effects.
(7) Robust standard errors in parentheses and *p*-values in brackets.
(8) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

the women answered on topics such as attitudes women's education, whether women should work and earn an income and women's mobility.[11] For the average worker in the sample who works 18 months, empowerment increases by 0.14 standard deviations.[12] This may seem small in magnitude, but it is worth keeping in mind that social and cultural gender norms are hard to change. The effects are comparable in magnitude to the effects on gender attitudes found in other work. For example, Jensen and Oster (2009) find that adding cable television is associated with a 0.19 standard deviation improvement in women' autonomy and decision-making, a 0.19 standard deviation decrease in the number of situations in which beating is considered acceptable and a 0.12 standard deviation decrease in the likelihood of wanting the next child to be a boy. In results available on request, I find that these results hold even when I restrict the analysis to the sample of unmarried women.

Column (2) shows that women who have worked longer have a higher internal locus of control, with degree of internal locus of control increasing by 0.01 standard deviations for every month worked, or 0.18 standard deviations for the average worker in the sample. The locus of control measure is constructed using responses to a series of statements about the degree to which workers' agree or disagree on whether they can control their life events with their actions (high internal locus of control) or whether life events depend on outside factors (low internal locus of control) (Rotter 1966).[13] While the increase in the locus of control is modest, in interpreting the magnitude, it is important to note that these measures are considered to be determined during childhood and to stabilize

[11]The women were asked whether they agree or disagree with several statements on the role and status of women. Section A2 provides details on how this index was constructed.

[12]Although not reported in the paper, I also find that this increase is stronger if I restrict the statements to those about education and economic opportunities for women.

[13]I use five standard statements used in measures of locus of control. The responses to each statement were independently coded for whether agreeing indicates a higher or lower internal locus of control. Section A2 in the appendix provides further details on the questions comprising the index.

during adolescence and hence may be hard to move (Weisz and Stipek 1982 provide a review).[14] The results suggest that formal employment gives women more confidence and independence in their ability to influence outcomes.

I next consider an index of marriage decisions and attitudes that asks women the earliest age they would consider getting married and whether they would be allowed to refuse a marriage proposal. These are particularly relevant to understanding how working could affect marriage outcomes. The results in column (3) show that for every month worked, women are 0.03 standard deviations more empowered in the marriage decision.[15] This is a fairly large effect translating to more than half a standard deviation for the average worker in the sample. In a setting like India, where arranged marriages are the most common types of marriage and many women report meeting their spouses on the day of their wedding, the ability to influence marriage outcomes such as refusing a marriage proposal is uncommon and represents a significant increase in autonomy for a woman.

Finally, I consider the impact on labor supply decisions. Less than 25 percent of the sample report currently working. This includes any type of employment including casual labor within the village. In results available on request, I find that this does not differ by duration exposed to the fixed-term contract. In column (4), I show the effect of working on a work autonomy index. The index includes two questions on the reason the woman stopped working and the person she thinks should control her earnings.[16] I find that working increases the autonomy women have in labor supply decisions by .03 standard

[14]The most comparable evidence to calibrate the magnitude is from Gottschalk (2003). He documents an increase between 0.05 and 0.1 on the probability of disagreeing with statements indicating an external locus of control following an increase in work by 361 hours through a tax credit for welfare recipients.

[15]Section A2 in the appendix describes this index and Table A4 shows the effects on individual components of this index.

[16]Section A2 in the appendix provides more details on the index and Table A4 shows the effects on each component of the index.

deviations for every month worked.

Overall, the results in Table 1.7 indicate that working increases empowerment and autonomy. Moreover, in analysis not included in the paper, I find a positive correlation between age of marriage and empowerment suggesting that increases in empowerment may be a plausible channel for the effects seen on marriage and fertility.

1.5.2 Household Wealth

When women work, they contribute to overall household income and wealth. Total household income increases may be associated with effects on marriage, fertility and younger siblings even if the women are not more empowered. Table 1.8 shows the impact of working on different measures of wealth for the woman's current household. This is the household the woman currently lives in, which is typically the spouse's household for married women and the parental household for unmarried women.

The mean household income in the sample is approximately Rs. 4900 a month (less than $100 a month). Column (1), shows there is only a small positive, but insignificant effect on current household income. I can reject an increase greater than 3 percent with 95 percent confidence. Column (2) and (3) show that there is a small negative (but insignificant) effect of time spent working on savings and loans. This includes savings in formal institutions as well as savings in the form of gold or jewelry and loans from both formal and informal institutions. I can reject an increase in savings of more than Rs. 1000 and a decrease in loans of more Rs. 4000 with 95 percent confidence. Finally, there is a small positive but insignificant effect on the number of household assets the woman reports having in her household and I can reject an effect size of greater than .09 assets with 95 percent confidence. Overall these results do not provide conclusive evidence that a household wealth effect is an important channel for the effects.

Table 1.8: *Household Wealth*

	Log Household Income	Savings	Loans	HouseHold Assets
	(1)	(2)	(3)	(4)
Panel A: IV Results				
Months worked	0.0138	-2988.1	-585.5	0.0324
in factory	(0.00919)	(2018.7)	(1730.2)	(0.0310)
	[0.132]	[0.139]	[0.735]	[0.297]
Panel B: Reduced Form Results				
Months before X 3 year	0.00681	-1438.6	-318.6	0.0158
	(0.00445)	(972.6)	(957.5)	(0.0153)
	[0.126]	[0.140]	[0.739]	[0.301]
Sample Mean	4880.0	57873.3	27873.3	5.683
Observations	955	702	605	981

Notes:
(1) Columns (1) to (4) of Panel A show the IV results for the impact of working outside the household on household wealth.
(2) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(3) Columns (1) to (4) of Panel B show the reduced form results for the effect of duration under the old contract on on household wealth.
(4) Individual controls for age and education and cohort of joining fixed effects included.
(5) Results are consistent with dropping cohort of joining fixed effects.
(6) Some respondents were not able to provide the value of savings and loans and hence the sample size in columns (2) and (3) is smaller.
(7) Robust standard errors in parentheses and *p*-values in brackets.
(8) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

1.6 Conclusion

Policies that increase the age of marriage and decrease fertility are particularly interesting for researchers and policy-makers. This paper provides evidence that working outside the village leads to higher female empowerment and autonomy, which translate into changes in real outcomes such as delaying the age of marriage and lower desired fertility. For every month worked, the probability of being married by age of 21 decreases by 0.01 and age of marriage increases by 1.1 months. Moreover, the effects are not restricted to the women who work, but there are positive externalities to age of marriage and education of younger siblings. These effects on younger siblings occur without the siblings themselves changing their work behavior.

The empirical analysis in this paper uses a change from fixed-term wage contracts to daily wage employment as a source of variation for duration worked at the firm. It is an open question why the change in contract had such a strong effect on duration worked. The women could have continued to work at the factory and even replicated the savings provided by the fixed-term contract themselves. This suggests that there may be other factors that affect length of employment for women. For example, it may be the case that the same barriers that lead to low female empowerment also prevent women from working in the absence of incentive-based contracts. Alternatively, existing literature highlights that when workers transition from traditional work to factory work they lack discipline and self-control, and may need contracts to overcome these behaviors (Clark 1994, Kaur, Kremer and Mullainathan 2010). The setting in this paper suggests that first-generation workers in the manufacturing sector may suffer from discipline problems in the duration of employment. Further research is required to understand whether the reason for this is an external barrier to working or internal problems with discipline. However, irrespective of which of these factors lead to a decreased length of employment, the findings in this paper suggest that providing employment opportunities

may not be sufficient to encourage women to stay in the formal labor market. In addition to providing employment opportunities for women, policymakers intending to increase female labor supply should consider policies that also provide incentives to work.

Chapter 2

How does Child Labor respond to changes in Adult Work Opportunities? Evidence from NREGA[1]

2.1 Introduction

Workfare programs in many developing countries aim to reduce poverty by functioning as conditional cash transfers. Typically such programs do not directly target children, but have the potential to improve outcomes for children by increasing household income and financial security. However, these programs can also have perverse effects on children by changing the rural economy and time allocation of household members. This paper studies the impact of a large workfare program in India on schooling and employment outcomes for children.

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA), passed in 2005 in India, has created one of the largest public works programs in the world. NREGA

[1]Co-authored with Mahnaz Islam, Harvard Kennedy School

offers 100 days of guaranteed work to rural households with the intention of helping households smooth consumption during lean agricultural seasons. NREGA targets the household, rather than individual members and NREGA work can only be taken up by adults. While NREGA increases household income and can increase education for children, it also increases wages in the rural economy, thus changing the opportunity cost of schooling for children. Moreover, it can cause other changes both in the rural economy and within the household by changing time allocation decisions of adults and bargaining power of women. Therefore, the impact of NREGA on both children's schooling and labor market decisions is an empirical question.

We use several rounds of nationally representative cross-sectional data from the National Sample Survey (NSS) in India. We exploit the phased roll-out of NREGA to different districts and measure the difference-in-difference between districts that received the program early relative to those that received it later. We find that time spent in public works increases for both adult men and women, which is consistent with findings from other papers (for example, Imbert and Papp 2013). Moreover, wages for casual work (non-NREGA casual labor) increase for adult men and women. For children, we show that when NREGA work is introduced to a district, younger children (ages 6 to 9) experience a 3 percent increase in time spent on education and older children (ages 15 to 17) experience an 18 percent increase in time spent working outside the household.

However, with the cross-sectional NSS data we can not tell whether the impact of NREGA we measure is for adults and children from the same or from different households. Therefore, as a robustness check, we use panel data from three states also collected by the NSS. We look at how time use for children changes during weeks when adults take-up NREGA work. The results from the panel data are consistent with the results from the cross-sectional data, and suggest that the impact of NREGA for adults and children that

we observe are likely to be from individuals within the same household. When adult time in public works in a given week increases, time spent by younger children in education increases and time spent by older children working outside the household increases.

The main results support a model where the income effect of NREGA is stronger for younger children for whom the wage change due to NREGA is unlikely to matter. The substitution effect due to the wage increase is stronger for older children and increases the opportunity cost of schooling. However, a simple back of the envelop calculation suggests that the wage elasticity of labor supply for older children is 4.4, which is implausibly high compared to estimates from other settings and suggests other channels for the increase in labor supply by older children.

It may be the case that new jobs which were previously not available for children due to job rationing open up when some of adult labor is used for NREGA. This is consistent with the results from the panel data that show time spent by older children doing outside work increases in weeks that parents work in NREGA. Another mechanism that could explain the magnitude of the wage elasticity could be that adults spend less time working in household enterprises when NREGA jobs open (which we observe in the data), and there may be strong complementarities between adult and child work in household enterprises leading to older children spending more time working outside the household rather than in the household. This is also consistent with the panel data results which show a positive correlation between adult and child time in household enterprise work, and a decrease in time spent by older children in household enterprise work in weeks that adults work in NREGA.

This paper adds to the growing body of literature evaluating the impact of NREGA (Ravi and Engler 2009, Sharma 2009, Azam, 2011, Afridi et al. 2012, Zimmerman 2012,

Imbert and Papp 2013 etc). However, we focus on the effects on children, who are non-participants in the program. The closest work to ours is by Afridi et al. (2012) which finds that greater participation of mothers in NREGA is associated with better educational outcomes for their children by empowering mothers through better labor opportunities for women. However, this is the first paper that studies NREGA's different effects by age group on children.

This paper also contributes to the literature on promoting education for children and reducing child labor which have been key policy issues in developing countries. Research on conditional cash transfers (CCTs) have shown that CCTs can reduce outside work for children (Schultz, 2004) and domestic work for girls (Skoufias et al., 2001). Studies on unconditional cash transfers have also established that such transfers can delay entry into paid employment for children (Edmonds & Schady, 2009) and have a positive, although smaller, impact on schooling (Baird, McIntosh & Ozler, 2011). However, when an income increase for the household is not due to a pure transfer, but rather some other economic shock, changes to child labor often depend on changes in adults' activities due to the shock as well as any changes in the local economy.

Finally, this paper contributes to the literature on targeting. Many policies and programs targeting children have focused on women. We find that a workfare program that targets the household rather than specific individuals can have positive effects on children. However, the different effects on older and younger children suggest that careful consideration should be given to potential spillovers when designing programs.

The rest of this paper is structured as follows. Section 2.2 describes the background and details of NREGA. In Section 2.3, we provide a simple conceptual framework to explain the differential effects on education and child labor by age group. Section 2.4 describes

the data and the estimation strategy. Sections 2.5 and 2.6 present the main results and robustness checks. We discuss alternative mechanisms in Section 2.7 and conclude with a policy discussion in Section 2.8.

2.2 Background on NREGA

The Mahatma Gandhi National Rural Employment Guarantee Act (NREGA) was enacted in 2005 and it guarantees 100 days of wage employment work per financial year to every rural household in India. Although the law was passed in 2005, the act was not made applicable to all districts at the same time. It was first phased into 200 districts in February 2006. An additional 130 districts were included in April 2007 for the second phase and the remaining 284 districts were included in April 2008[2]. Within each state, the earlier districts were chosen because they were identified as backwards and least developed (NREGA Report to the People, 2010).

Once the program is available in a district, each rural household is entitled to 100 days of guaranteed wage employment in a financial year, if adult members in the household are willing to do unskilled manual work under the program. To enroll in the program, a household registers with the Gram Panchayat (village-level self governing body) and is issued a Job Card. Job Card holders can then apply for work to the Gram Panchayat and are entitled to receive work within 15 days of the application. If they do not receive work within that time, households are supposed to receive unemployment insurance, although this aspect of the program is not well implemented. Although the program targets households rather than individuals, it promotes participation of women in wage employment. According to the Act, at least one-third of workers hired under the program

[2]Information retrieved from NREGA website. Phase in dates and list of districts compiled from http://nrega.nic.in/MNREGA_Dist.pdf and http://nrega.nic.in/circular/Report_to_the_people.pdf respectively.

must be women (NREGA, 2005).

Since poverty alleviation is the main focus of the NREGA, it is often compared to a cash transfer program (Papp & Imbert 2013, Kapur et al. 2008). Moreover, workers are paid wages at the state-wise specified wage rates for the program, which are usually higher than prevailing agricultural wages. Several papers document an increase in private sector wages for men and women (Imbert & Papp, 2012; Berg, Bhattacharyya, Durgam, & Ramachandra, 2012) and only for women (Zimmerman, 2012) as a result of the program. Thus the program can be considered to have two effects on the rural economy - it increases income and the wage rate for households.

2.3 Conceptual Framework

In this section we provide a simple conceptual framework to understand the impact of NREGA on a rural household, specifically for child labor. Following Basu, Das & Dutta (2007), we model a unitary household with one adult and one child[3]. We use a unitary model of the household since NREGA is targeted at the household rather than at an individual, but we make a distinction between adults and children because children can not work in NREGA jobs.

The household derives utility from consumption. We assume adult labor is costless. However, child labor is costly and the opportunity cost is the time spent in school. Utility is given by the following quasi-linear utility function:

$$U(c, l) = \phi(c) - \alpha l$$

[3]Intra-household dynamics may change since NREGA provides women with a chance to work which could increase their bargaining power. However, for the purpose of this basic model, we simplify and do not use a collective household model.

where c is household consumption, l is the time spent by children working, $\phi'(c) \geq 0$, $\phi''(c) \leq 0$ and α is a positive real number.

This utility function satisfies the *Luxury Axiom*, which is defined as "a family will send the children to the labor market only if the family's income from non-child-labor sources drops very low". Adults supply a fixed time to the labor market T. We assume the price of the consumption good is 1 and wages for adults and children are w and w^C respectively. The budget constraint is given by

$$c \leq w^C l + wT$$

The household problem is given by:

$$\max_l \left\{ \phi(w^C l + wT) - \alpha l \right\}$$

We assume a perfect labor market with one sector (agriculture). Children work in this sector, however, they are less productive and their productivity is a function of their age, a. One unit of child labor is $p(a)$ units of adult labor, where $0 \leq p(a) \leq 1$ and p is an increasing in a. Older children are more productive than younger children and are therefore more substitutable for adult labor. Wages w and w^C are such that $w^C = p(a)w$.

The household problem can be now expressed as

$$\max_l \{\phi(p(a)wl + wT) - \alpha l\}$$

This gives us the first order condition

$$p(a)w\phi'(p(a)wl + wT) = \alpha$$

Differentiating implicitly with respect to w and rearranging the terms, we get

$$\frac{dl}{dw} = -\frac{\phi' + w(T + p(a)l)\phi''}{p(a)w^2\phi''} \tag{2.1}$$

Labor supply for children increases with w when the following condition holds:

$$p(a) > -\frac{\phi'}{wl\phi''} - \frac{T}{l} \tag{2.2}$$

Since $p'(a) \geq 0$, this conditional is more likely to hold when age, a, increases. Thus, older children are more likely to respond to an increase in wages by increases their labor supply than younger children.

When NREGA work is introduced into the rural economy, another sector (public sector) opens, but only adults can work in this sector. NREGA wage is set at $\bar{w}$ where $\bar{w}$ is greater than the pre-NREGA wage in the economy. Moreover, days of NREGA work are capped at 100 days. Since public sector wages are higher than agriculture sector wages, adults will shift to the public sector. But they will only work a maximum of 100 days there and spend any additional time working in agriculture. This shifts the labor supply curve in the agriculture sector to the left, as adults spend less time in the sector. This shift in labor supply increases wages in the agricultural sector.

Higher wages and household income from NREGA have two effects on children. While higher household income reduces child labor supply and increases schooling through an income effect, children also respond to higher wages and spend more time working through the substitution effect. Equation 2 implies that the substitution effect is more likely to be true for older children since the increase in wages is larger for older children.

For younger children the income effect is more likely to dominate. We will test this empirically in the following sections.

2.4 Data & Estimation Strategy

We use four rounds of nationally representative cross-sectional employment data collected by the National Sample Survey Office (NSSO) starting in 2004 and until 2008. The NSSO Employment and Unemployment survey is conducted from July to June in order to capture one full agriculture cycle and is stratified by urban and rural areas of each district. Since the NREGA is only applicable for individuals living in rural areas, we drop the urban population in our analysis. We include all districts from all states in India, excluding Jammu and Kashmir since survey data is missing for some quarters due to conflicts in this area. The NSSO over-samples some types of households and therefore all estimates are computed adjusted using the sampling weights provided by the NSSO.

Our data spans January 2004 to January 2006 to form the pre-program period and July 2007 to June 2008 for the post-program period. To define the pre-program and post-program periods, we obtained data on the NREGA phase-in by district from the NREGA website. We use the individual as our primary unit of analysis. Table 2.1 provides summary statistics for the pre-program period from the 60th round of the NSS data.

Our main outcomes are individual-level measures of time spent on various activities in the last seven days for adults as well as children. The NSSO Employment and Unemployment surveys collect data at the individual level on activities undertaken in the last seven days at the time of the survey by each household member over the age of four. For each day and each activity, the survey records whether the activity was performed at an intensity

Table 2.1

Summary Statistics: NSS 60th Round

Average Number of Children by Age Group:	
Age 6 to 17	1.365
	(1.391)
Age 6 to 9	0.509
	(0.747)
Age 10 to 14	0.582
	(0.827)
Age 15 to 17	0.275
	(0.525)
Individual & Household Characteristics:	
Age	25.797
	(19.333)
Fraction literate	0.530
	(0.499)
Fraction married	0.461
	(0.498)
Fraction widowed	0.046
	(0.209)
Fraction divorced	0.002
	(0.044)
Fraction in scheduled caste tribe	0.742
	(0.437)
Fraction Christian	0.019
	(0.137)
Fraction Muslim	0.105
	(0.307)
Household size	6.201
	(2.963)

of 0, 0.5 or 1 day. Using this data we construct variables on number of days spent by each household member in the past week on public works, non-public outside work, work on household enterprise, domestic activities and all other activities. For children, we separate out number of days spent on educational activities and we only have one category of outside work since children cannot work in public works. The activities are mutually exclusive and the total adds up to 7 days for each individual.

The survey also asks total earnings in the past seven days for individuals who worked in casual labor. Our wage measures use this data to compute average earnings per day worked in non-public casual labor.

Our empirical strategy follows Imbert and Papp (2013) and uses the phased roll-out of the NREGA to different districts and compares changes in districts that received the program earlier to districts that received the program later. The program was introduced to 200 districts in February 2006 as part of the first phase, to 130 districts in April 2007 as part of the second phase and to all remaining districts in April 2008. We compare individuals from districts in the first two phases to individuals from districts that received the program in the final phase.

However, a simple comparison of individuals from districts that received the program in different phases is biased by the fact that districts in the earlier phases are more backward than those in later phases on socio-economic characteristics such as agricultural wages and output which directly affect labor market outcomes. To address this concern we compare changes over time in districts that received the program earlier to those that received it later and include district fixed effects.

We use the following difference-in-difference specification comparing Phase I & II districts

to Phase III districts before and after NREGA is rolled out for Phase I & II districts:

$$y_{idt} = \beta_0 + \beta_1 nreg_{dt} + \gamma X_{idt} + \mu_d + \eta_t + \epsilon_{idt} \tag{2.3}$$

where y_{idt} is days spent in education, labor, domestic activities, etc for an individual i in district d at time t. The variable $nreg_{dt}$ is 1 if at the date of the survey, NREGA was available in district d and is 0 otherwise. X_{idt} is a set of individual and household level variables including age, age squared, literacy, religion, social group, and household size. We also include district fixed effects (μ_d) and quarter-year fixed effects (η_t). We re-weight observations using sampling weights and cluster standard errors at the district level. The coefficient β_1 gives the effect of NREGA on days spent in each activity by individual i.

2.5 Results

2.5.1 Changes to the Rural Economy

Table 2.2 shows the changes in time spent in various activities by adult men and women. Once NREGA is rolled into a district, casual public work by men increases by 0.055 days in the last seven days. For women, time spent in casual public work increases by 0.032 days in the last seven days. Both these coefficients are significant at the 1 percent level. Mean days spent on casual public work before NREGA is 0.021 days by men and 0.010 days by women, so this a very large increase; time spent in casual public work approximately doubles for men and triples for women.

For men, the increased days spent in casual public work mostly comes from a reduction in time spent working in household enterprises. The number of days spent working in household enterprises by men decreases by 0.190 days in the last seven days, and the coefficient is significant at the 1 percent level. For women, the increased time in casual

Table 2.2

Number of Days Spent by Adults on Different Activities in the last 7 days:

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Casual Public	(2) Non-Public Outside	(3) HH Enterprise	(4) Domestic	(5) Other
Panel A: Men					
NREG	0.055***	0.087	-0.190***	-0.011	0.059
	(0.012)	(0.060)	(0.063)	(0.014)	(0.037)
Observations	315,371	315,371	315,371	315,371	315,371
Non-NREG mean	0.021	2.283	3.474	0.106	1.116
Panel B: Women					
NREG	0.032***	-0.060*	0.035	-0.051	0.045
	(0.009)	(0.035)	(0.054)	(0.062)	(0.027)
Observations	314,630	314,630	314,630	314,630	314,630
Non-NREG mean	0.010	0.830	1.479	4.136	0.545

Note: Includes controls for age, age^2, literacy, marital status, household size, religion and social group. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.3

Log of Daily Casual Wages (Non-Public)

Includes District & Year*Quarter Fixed Effects

	Adults: 18 to 60		
	All	Women	Men
	(1)	(2)	(3)
NREG	0.041***	0.053**	0.035**
	(0.016)	(0.024)	(0.015)
Observations	79,199	22,041	57,158
Non-NREG mean	55.43	39.70	62.20

Note: Includes controls for age, age^2 household size, literacy, marital status, religion, social group. Standard errors, adjusted for clustering, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

public work comes from a reduction in time spent on non-public outside work (decrease of 0.060 days in the last seven days, significant at the 10 percent level), and also from a reduction in time spent in domestic work (decrease of 0.051 days in the last 7 days), although this coefficient is not significant at the 10 percent level.

While the percentage increase in days spent in public works is large, the magnitude of the change in terms of days spent in a year is small, approximately 2.9 days per year for men and 1.7 days for women. However, this averages over all rural households regardless of participation. Estimate of average days worked by participating households is much higher and according to the official website, in 2010-11 the NREGA provided 2.27 billions person-days of employment to 53 million households (Papp and Imbert 2013).

Table 2.3 shows changes in log of daily casual wages (from non-NREGA work), once the program comes into the district. Overall, wages increase by 4.1 percent, and the coefficient is significant at the 1 percent level. Disaggregating by gender, we see that wages for women increase by 5.3 percent and wages for men increase by 3.5 percent. Both

Table 2.4

Number of Days Spent by Children on Education in the last 7 days:

All Children: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) All Children	(2) Age 6 to 9	(3) Age 10 to 14	(4) Age 15 to 17
Panel A: All Children				
NREG	0.029	0.184***	-0.015	-0.194**
	(0.049)	(0.069)	(0.059)	(0.099)
Observations	294,484	100,422	127,366	66,696
Non-NREG mean	5.384	5.875	5.741	3.748
Panel B: Boys				
NREG	0.028	0.200**	-0.051	-0.221*
	(0.056)	(0.080)	(0.067)	(0.129)
Observations	294,484	100,422	127,366	66,696
Non-NREG mean	5.601	6.014	6.017	4.063
Panel C: Girls				
NREG	0.040	0.164*	0.031	-0.190
	(0.059)	(0.084)	(0.078)	(0.127)
Observations	137,101	47,698	59,421	29,982
Non-NREG mean	5.132	5.721	5.418	3.341

Note: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

coefficients are significant at the 5 percent level.

Thus household income increases from both wages earned from NREGA work and from higher wages from non-NREGA work. Moreover, as Appendix Table B1 shows, changes in total days worked by the household also increases further increasing household income.

2.5.2 Effect on Time Use by Children

The increase in family income can change time spent by children in schooling. Panel A of Table 2.4 shows the effect on time allocation towards education when NREGA is rolled in

to a district. Column 1, pools children of all age groups together, and we see that there is no significant impact on time spent in education. However, when we disaggregate by age group, we see strong effects in opposite directions for the youngest and oldest age groups. Children between ages 6 to 9 years, spend 0.184 days more in the past week (significant at the 1 percent level), and children aged 15 to 17 spend 0.194 days less, in the past week, in schooling (significant at the 5 percent level). The coefficient is not significant at the 10 percent level for children in the middle age group of 10 to 14 years. Panels B and C of Table 2.4, shows the results by gender. The results for boys are stronger, although the coefficients for girls are similar in magnitude and direction but less precise.

Table 2.5 shows the effects on labor market and activities other than education by children, when NREGA is introduced to a district.[4] Panel A shows the effects on children aged 15-17 years. When NREGA in introduced to a district, children in this age group spend 0.131 days more working outside the household in the last seven days. This coefficient is significant at the 5 percent level. This represents an 18 percent increase in time spent working outside the household for children in this age group. The coefficients for the remaining activities are not precise. Overall, the results show that 15-17 year old children spend more time working for a wage, at the expense of time spent in education.

For children in the youngest age group (ages 6-9 years, shown in Panel C), time spent in "other" activities decreases once NREGA comes in. In the past seven days, the youngest children spend 0.197 days less in other activities. This category is coded as anything other than time spent in domestic work, household enterprise work, outside work or education and we interpret it as leisure. The coefficient for outside work for the youngest children

[4]Note that the time spent on the different activities including education adds up to seven days for each child.

Table 2.5

Number of Days Spent by Children on Different Activities in the last 7 days:

All Children: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.131**	-0.090	0.088	0.065
	(0.061)	(0.064)	(0.074)	(0.052)
Observations	66,696	66,696	66,696	66,696
Non-NREG mean	0.728	0.887	1.083	0.554
Panel B: Age 10 to 14				
NREG	0.007	0.025	0.044	-0.062
	(0.016)	(0.034)	(0.038)	(0.039)
Observations	127,366	127,366	127,366	127,366
Non-NREG mean	0.124	0.196	0.432	0.507
Panel C: Age 6 to 9				
NREG	-0.005*	0.002	0.016	-0.197***
	(0.003)	(0.010)	(0.020)	(0.065)
Observations	100,422	100,422	100,422	100,422
Non-NREG mean	0.004	0.014	0.056	1.052

Note: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

is also negative, but as very few children in this age group work, this coefficient should be interpreted cautiously. As before, the coefficients for children in the middle age group are not precise.

Overall, the results in tables 2.4 and 2.5 show that when NREGA is introduced to a district time spent in education increases for the youngest children and time spent working for a wage outside the household increases for the oldest children. Tables B2 and B3 in the Appendix, show results separated by gender. Consistent with previous results for education, we find that the changes in time spent by boys and girls are similar, but that the coefficients for girls are less precise.

2.6 Robustness Checks: Further Evidence from Panel Data

The data used for the previous results are several rounds of cross-sectional data from the NSSO which does not allow us to observe the same household over time. We thus cannot differentiate whether the effects we observe are for adults and children from the same or from different households. In this section we provide evidence from panel data for three states collected by the NSSO.

2.6.1 Data & Estimation Strategy

The NSSO conducted a panel survey with a focus on NREGA spanning the years from 2009 to 2011. At this time, the NREGA had been introduced in all districts. The sample consisted of 912 villages in Andhra Pradesh, Madhya Pradesh and Rajastan. The survey included four rounds of the Employment and Unemployment surveys in the same format at the cross-sectional surveys. Each household was visited four times over two years between July 2009 to June 2011. Table 2.6 provides summary statistics on household composition for children in different age groups in the panel data.

Table 2.6

Summary Statistics: Panel Data Household Composition

Household Composition in Visit 1	
Percentage of Households with Children age 6 to 17	57.8
Percentage of Households with Children age 6 to 9	32.2
Percentage of Households with Children age 10 to 14	35.9
Percentage of Households with Children age 15 to 17	22.7
Percentage of Households with Children in both groups:	
Age 6 to 9 & Age 10 to 14	18.1
Age 10 to 14 & Age 15 to 17	13.7
Age 6 to 9 & Age 15 to 17	6.4
Percentage of Households with Children in all three groups	5.1

Since the panel data was collected after NREGA was available in all districts, we do not have variation in NREGA work availability within the sample. We instead use the panel data to look at the response of time allocation by children within the household when adults take up NREGA work. This allows us look at whether the response by children that we observe in the cross sectional data is likely to be children from the same households or from different households as the ones where adults work in NREGA. We use the following specification with household fixed effects:

$$y_{ht} = \beta_0 + \beta_1 CasualPublicDaysAdults_{ht} + \beta_2 X_{ht} + \gamma_h + \delta_t + \mu_{ht} \tag{2.4}$$

where y_{ht} is the household aggregate of time spent on each activity by children from household h at time t and $CasualPublicDaysAdults_{ht}$ is the total number of days spent by the adults in the household on casual public work in the last seven days. β_1 is the change in time allocation by children in the household when time spent working in NREGA jobs by adults in the household changes.

2.6.2 Results: Changes within Household

Table 2.7 estimates the changes in time allocation by children within a household in weeks when adults spend more time in casual public work. Panel A presents the results for children between the ages of 15 to 17 and shows that one additional day of work in the past seven days by adults in casual public work results in children working outside by 0.038 days more during that period (significant at the 5 percent level). Additional time working outside is reallocated from less time working in household enterprises (a decrease of 0.027 days in the last week, also significant at the 5 percent level).

For the younger children, in age groups 10-14 years and also 6-9 years, additional time spent by adults in casual public work is related to children spending more time in education (Panels B and C). For each additional day spend by adults in casual public work in the last seven days, 10-14 year olds spend 0.018 days extra in school (significant at the 5 percent level) and 6-9 year olds spend 0.013 days extra in school (significant at the 1 percent level) during that time. For the youngest children, as in the cross-sectional data, the extra time in education mostly comes from time otherwise spent in “other" activities, that we interpret as leisure.

Overall, these results are consistent with the results from the cross-sectional data showing increases in time spent in education for younger children and increases in time spent working for older children. Further, since we observe the same household over time in the panel data, these results suggest that the changes seen in educational and outside work for younger and older children likely come from the same households where parents work in NREGA.

Table 2.7

Number of Days Spent by Children on Different Activities in the last 7 days during Adult NREG Work (Panel): (Household Aggregates)

Includes Household Fixed Effects

VARIABLES	(1) Education	(2) Domestic Work	(3) HH Enterprise	(4) Working Outside	(5) Other
Panel A: All Children: Age 15 to 17					
# Days in Casual Public Work (adults)	0.004	-0.006	-0.027**	0.038**	-0.003
	(0.008)	(0.010)	(0.012)	(0.015)	(0.008)
Mean of dependent variable	4.40	1.23	1.07	0.82	0.45
Observations	15,722	15,722	15,722	15,722	15,722
Panel B: All Children: Age 10 to 14					
# Days in Casual Public Work (adults)	0.018**	-0.007	0.006	-0.002	-0.011
	(0.007)	(0.005)	(0.005)	(0.003)	(0.007)
Mean of dependent variable	8.37	0.78	0.45	0.16	0.48
Observations	25,176	25,176	25,176	25,176	25,176
Panel C: All Children: Age 6 to 9					
# Days in Casual Public Work (adults)	0.013*	-0.001	-0.001	0.001	-0.010
	(0.008)	(0.003)	(0.002)	(0.001)	(0.008)
Mean of dependent variable	8.31	0.15	0.05	0.01	0.83
Observations	22,589	22,589	22,589	22,589	22,589

Standard errors clustered at the household level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

2.7 Alternative Mechanisms

The introduction of NREGA results in two opposing effects on child labor and education that vary by age. Our results from the cross-sectional and panel data suggest that the income effect of NREGA dominates for the youngest children for whom wage is unlikely to increase. Considering their age group of 6 to 9, we can even assume that the entire effect is an income effect since children in this age group are very unlikely to work outside for wages. For older children, there is a strong substitution effect from the increase in wage and the substitution effect dominates the income effect.

The estimates in Section 2.5 show an increase in labor supply of 18 percent for older children from a wage increase of 4.1 percent for adults. This suggests a wage elasticity of child labor supply of 4.4. This is likely to be an underestimate since the wage increase for children is likely to be smaller and the effect on older children is a net effect of the income and substitution effect of NREGA. This magnitude is implausible in this context and given findings in other studies (Grootaert and Kanbur 1995). This suggests that there may be other channels that are important consider.

2.7.1 Existence of Surplus Labor

Labor markets in rural India are likely to be imperfect, and may be characterized by the presence of surplus labor. In such a scenario, before the availability of NREGA, older children could have wanted to work outside the household but may have unable to find work as the labor market did not clear. When NREGA is introduced into a district, job opportunities open up for children since adults now spend some of their time doing NREGA work. Therefore, older children do not simply respond to higher wages, but are now able to work more outside due to increased job availability. While we cannot test this directly using the data, the large estimates for wage elasticity of child labor supply

suggest that this is a possible channel.

2.7.2 Changes in Household Enterprise work

Changes in time allocation by adults can also directly change time allocation for children if work by children in the household is a substitute or complement for work by adults. Table B1 in the Appendix and Table 2.2 show that both the household and adult men in particular spend less time working in household enterprises (predominantly agricultural activities in our sample) once NREGA enters a district. A reduction in time spent working in household enterprises by adults can free up time for children if household enterprises if there are strong complementarities to adult and child time in household enterprises. If this is the case, older children may now take up jobs outside the household and younger children may spend more time in school. Moreover, if parents are more flexible as employers compared to outsiders, it may also explain the reduction in time spent in schooling by older children, if they now have to skip school more.

While it is difficult to test this more rigorously with the available date, Table 2.8 explores the relationship between time spent by adults in household enterprises and time allocation of children in the panel data sample. We use an estimation similar to Section 2.6 replacing time spent by adults in public works with time spend by adults in household enterprise work. We see that there are strong complementarities between time spent by adults and time spent by children in household enterprises and the results are the strongest for the oldest children. For each additional day spent by adults in household enterprise work in the last seven days, children in the age group 15 to 17 spending 0.065 days more working in the household enterprise (significant at the 1 percent level). This suggests that some of the increase in outside work by older children may come from a shift away from household enterprise work.

Table 2.8

Number of Days Spent by Children on Different Activities in the last 7 days during Adult HH Enterprise Work (Panel):

Includes Individual Fixed Effects

VARIABLES	(1) Education	(2) Domestic Work	(3) HH Enterprise	(4) Working Outside	(5) Other
Panel A: All Children: Age 15 to 17					
# Days in HH Enterprise Work (adults)	-0.002	-0.019***	0.065***	-0.032***	-0.012***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.003)
Observations	17,934	17,934	17,934	17,934	17,934
Mean of dependent variable	3.86	1.08	0.94	0.72	0.40
Panel B: All Children: Age 10 to 14					
# Days in HH Enterprise Work (adults)	0.001	-0.003	0.013***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Observations	36,798	36,798	36,798	36,798	36,798
Mean of dependent variable	5.72	0.53	0.31	0.11	0.33
Panel C: All Children: Age 6 to 9					
# Days in HH Enterprise Work (adults)	0.002	0.001	0.001*	-0.001	-0.003
	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	30,163	30,163	30,163	30,163	30,163
Mean of dependent variable	6.22	0.11	0.04	0.01	0.62

Standard errors adjusted for clustering at the individual level in parentheses

*** p<0.01, ** p<0.05, * p<0.1

2.8 Conclusion

The NREGA is one of the largest public works programs in a developing country that targets adults in rural households and is aimed at reducing poverty and financial security by improving employment opportunities for the household, particularly when other employment options are scarce. This paper provides evidence on the impact of such a workfare program on children. We find that the effect on children varies by age, with younger children potentially benefitting from the increased household income and spending more time in school, while older children respond by increasing labor supply which may be an unintended consequence of the program.

Various large-scale programs in developing countries target school attendance, particularly for young children, including conditional cash transfers (Behrman, et al. 2005; Schultz, 2004; Rawlings & Rubio, 2005), school feeding programs (Afridi, 2010; Bundy et al., 2009; Jomaa, 2011), female school stipend programs. (Chaudhury, & Parajuli, 2010; Raynor, & Wesson, 2006). NREGA is not a program that targets education of children directly, and also differs from other programs in that it targets the household rather than any specific member. Although NREGA promotes employment opportunities for women, it is not specifically targeted towards women in the household. However, as our results show, the spillovers to education for young children are potentially large. If the magnitudes for improvements in school attendance by NREGA are similar to that by other programs, it raises the need for further discussion on the need for targeting. Even if the magnitude is smaller, the results for younger children are comforting as it provides evidence that improved financial security for the household results in increased schooling and improved opportunities for young children.

On the other hand, although NREGA work is restricted to adults, we observe perverse effects on education for older children due to the changes it causes in the local

economy and time allocation within the households. Our results show that older children spend less time in school, as well as more time in the labor market, at least partly due to the higher wages caused by NREGA. Therefore, to promote schooling for older children further safety nets should be built into NREGA and into any similar programs. Moreover, our results suggests that when evaluating the effects of such programs it is important to take into account possible spillovers.

Chapter 3

Age of Marriage and Female Autonomy in India

3.1 Introduction

In many developing countries, marriage, for women, has historically been almost universal, and remains so even among recent cohorts (Das and Dey, 1998). Moreover, many traditional communities encourage early marriage for women. Age of marriage has received attention from researchers and policy makers because early marriage is associated with lower educational attainment, female autonomy and early childbearing, which, in turn, increases maternal and child mortality. However, despite legislation and other programs targeting early marriage, the average age of first marriage in many south Asian communities, stays closer to 20 for over 50 percent of the women.

Studying the impact of age of marriage is challenging because social and economic factors associated with early marriage are also correlated with those that affect later-life outcomes for women. Recent work from Bangladesh uses age of menarche as an instrumental variable for age of first marriage and finds that each additional year that marriage

is delayed is associated with 0.22 additional years of schooling, 5.6 percent higher literacy and increased use of preventive health services (Ambrus and Field 2008). In this paper, I replicate this strategy in a sample of women from rural India to study the impact of delaying marriage on later-life outcomes for women, particularly female autonomy.

The impact of delaying marriage may vary in different contexts because of differences in social norms and local marriage and labor markets. Higher age at the time of marriage can worsen later-life outcomes through worse matches in the marriage market if men prefer younger women. However, if women who marry later get more schooling, education can improve these outcomes, both through a direct effect of education and through higher education of spouses through positive assortative matching (Boulier and Rosenzweig 1984). Finally, dowry could be used as pecuniary transfers that help clear the marriage market (Becker 1981). If higher age at the time of marriage for women is considered undesirable in the marriage market, women may pay a larger dowry to compensate, and this could result in no difference in spouse quality and other later-life outcomes. The net effect of age of marriage is an empirical question and depends on how the marriage market responds.

Variation in the onset of puberty for women creates a physical barrier to entry into the marriage market. While social and cultural norms encourage parents to marry their girls off at a young age, age of menarche puts a lower bound on the age of first marriage. Following Ambrus and Field (2008), I use natural variation in the timing of first menstruation within the age range of 11 to 16 to create quasi-random variation in the age of first marriage. I find that for a one year increase in age of menarche, age of marriage increases by 0.47 years.

I first use the IV strategy to study the effect of age of first marriage on education

and find results comparable to Ambrus and Field (2008). I find an increase in years of education and literacy when age of marriage increases, with education increasing by 0.5 years and literacy by 4.3 percentage points for every year marriage is delayed.

Next, I study the impact of age of marriage on later-life outcomes, specifically, health and female autonomy. I find no significant increases in use of prenatal care (the coefficient is negative and insignificant) and significant decreases in various measures of female autonomy. These effects of age of marriage on health and autonomy differ from those in Bangladesh observed by Ambrus and Field (2008). It is likely that these differences are due to differences operating through the effect of age of marriage on the marriage market.

Due to lack of data on dowry, I can only directly look at the effects of age of marriage on measures of spouse quality. I find that women who are older at the time of marriage get married to spouses who are more educated. Their current monthly per capita consumption is also higher. However, these effects hold for literate women, but go in the opposite direction for illiterate women. Finally, while the spouse may be more educated and current monthly per capital consumption is higher, women who are older at the time of marriage consider their natal families to be economically better off than their spouse's family at the time of marriage.

These results suggests that changes in marriage market outcomes by age and education could result in different later-life outcomes. First, spouse quality is affected by age of marriage, and dowry does not seem to have eliminated differences in spouse quality, unlike in Bangladesh (Ambrus and Field 2008). Second, there is some improvement in later-life outcomes that comes through more educated women being matched to more educated spouses. This is consistent with other evidence of positive assortative matching

with respect to schooling (Boulier and Rosenzweig 1984). Finally, there is still a cost to being older on the marriage market, and older women marry into families whose initial economic status is worse than their own. In a context where most women do not work and where couples often co-reside with the husband's parents, attitudes and beliefs about the role of women and female autonomy are slow to change and may depend on the initial economic status of the spouse's family. The lower initial economic status of the spouse's family could then explain why these women fare worse with regard to outcomes like mobility.[1]

This paper adds to the large body of literature on matching in the marriage markets (for example, Boulier and Rosenzweig 1984, Epstein and Guttman 1984, Behrman, Rosenzweig and Taubman 1994, Burdett and Coles 1997). In particular, we study the effect of age of marriage on later-life outcomes for women. Age of marriage is often associated with positive outcomes - for example, when age of marriage increases due to changes in legislature that increases age of consent for all women, or due to an increase in labor market opportunities for women that increases the value and bargaining power of women (Dahl 2010). The results in this paper suggest that increasing age of marriage may worsen some later-life outcomes for women, and that in order to understand the full impact, it is important to understand how the marriage market responds.

The rest of this paper is organized as follows. Section 3.2 describes the data and sample. Section 3.3 discusses the identification strategy. Sections 3.4 and 3.5 present the main results and discuss possible mechanisms. Section 3.6 concludes with a policy discussion.

[1]Women who are older at the time of marriage are more likely to state that in their communities husbands beat their wives for going outside the house without permission, which also suggests more restrictive norms on mobility in the community.

3.2 Data

I use data from the India Human Development Survey 2005 (IHDS) which was collected jointly by researchers from the University of Maryland and the National Council of Applied Economic Research (NCAER), New Delhi. The survey collected data from 41554 households in 1503 villages and 971 urban neighborhoods across India between September 2004 and August 2005. The sample included all states and union territories in India except the small population living in the Andaman and Nicobar Islands and Lakshadweep.

Two surveys were administered to the households by a male and a female surveyor. The first survey was an interview with a knowledgeable informant (typically, the male head of the household) on the socio-economic status of the household, social capital and the employment and education of all household members. The second survey was an interview with an ever-married woman aged 15-49 on topics including health, education, marriage, fertility, gender relations in the household and in the community. The survey also collected immunization histories for the last-born children of these women for births after 2000. In addition, anthropometric measures (height and weight) were taken for children and their mothers and a reading, writing and math test were administered to children between ages 8 and 11.

The main outcome variables are from the interview conducted with the eligible ever-married woman for whom anthropometric measures are also available. I also use data from the household survey to create control variables. I use the household roster to match women with their spouses wherever the spouses live in the same household and use this data to create the spouse characteristics variables.

I restrict the main analysis presented in this paper to the rural sample. I also restrict

Table 3.1: *Summary Statistics*

Variable	Full Sample	Age of Menarche (11-13)	Age of Menarche (14)	Age of Menarche (15-16)
	(1)	(2)	(3)	(4)
Age	32.631	32.568	32.68	32.682
	(8.087)	(8.106)	(8.016)	(8.154)
Age at Menarche	13.684	.	.	.
	(1.122)			
Height (cms)	151.007	150.851	150.757	151.681
	(6.902)	(6.587)	(7.292)	(6.882)
Hindu	0.825	0.814	0.831	0.839
	(0.38)	(0.389)	(0.375)	(0.368)
Muslim	0.099	0.114	0.088	0.084
	(0.298)	(0.318)	(0.283)	(0.277)
Christian	0.024	0.032	0.02	0.016
	(0.154)	(0.176)	(0.141)	(0.124)
Scheduled Caste/	0.333	0.36	0.318	0.304
Scheduled Tribe	(0.471)	(0.48)	(0.466)	(0.46)
Observations	19655	8724	6487	4444

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show the means for the full sample and the sample split by age of menarche.

(3) Standard deviation in parantheses.

the sample such that age of menarche ranges between 11 and 16. The final sample used for the analysis has 19655 women. Column (1) of Table 3.1 provides summary statistics that describe the population. 82.5 percent of the households in the sample are Hindu, with 9.9 percent Muslim and 2.4 percent Christian. About 33.3 percent of households are classified as a scheduled caste or scheduled tribe (SC/ST). The average woman in the sample is 32.6 years old and reaches menarche at an age of 13.7 years.

3.3 Empirical Specification

I follow the specification used by Field and Ambrus (2008) and use the age of menarche as instrument variable to assess the causal effect of age of marriage on later-life outcomes for women. In India, traditionally most women are married only after they reach puberty.[2]Thus, age of menarche is a lower bound for age of first marriage since women very rarely enter the marriage market before puberty. The IV approach involves estimating the following two-stage model:

$$Y_{ij} = \beta_0 + \beta_1 M_{ij} + \beta_2 X_{ij} + \delta_j + \epsilon_{ij} \tag{3.1}$$

$$M_{ij} = \gamma_0 + \gamma_1 P_{ij} + \gamma_2 X_{ij} + \delta_j + \mu_{ij} \tag{3.2}$$

where Y_{ij} is the outcome of interest for individual i from district j, M_{ij} is i's age at marriage, and P_{ij} is i's age at menarche, which is the instrument used to identify (1) and X_{ij} are individual controls and δ_j is a set of district fixed effects. The controls include age, age squared, adult height, religion and caste.

Identification of the IV model requires a strong correlation between age of menarche and age of first marriage. Table 3.2 show the results from the first-stage regression of age of

[2]In the sample, more than 85 percent of women report being first married after reaching puberty.

Table 3.2: *First Stage Regression: Age of First Marriage*

	(1)	(2)	(3)	(4)	(5)
Age at	0.342***	0.474***		0.202***	0.419***
Menarche	(0.0202)	(0.0205)		(0.0270)	(0.0275)
Menarche 11			-2.295***		
			(0.205)		
Menarche 12			-1.883***		
			(0.101)		
Menarche 13			-1.382***		
			(0.0862)		
Menarche 14			-0.876***		
			(0.0811)		
Menarche 15			-0.429***		
			(0.0859)		
Age	0.0737***	-0.00319	-0.00326	0.199***	0.0378
	(0.0207)	(0.0179)	(0.0179)	(0.0283)	(0.0280)
Age Square	-0.00133***	-0.000415	-0.000414	-0.00290***	-0.000492
	(0.000319)	(0.000274)	(0.000274)	(0.000444)	(0.000426)
Height	0.0307***	0.0128***	0.0129***	0.0297***	0.0146***
	(0.00334)	(0.00293)	(0.00293)	(0.00428)	(0.00445)
Muslim	-0.135*	-0.279***	-0.279***	-0.606***	0.631***
	(0.0706)	(0.0790)	(0.0790)	(0.106)	(0.0923)
Christian	3.070***	0.480***	0.480***	2.578***	2.010***
	(0.144)	(0.154)	(0.154)	(0.148)	(0.307)
SC/ST	-0.559***	-0.440***	-0.438***	-0.334***	0.0160
	(0.0484)	(0.0440)	(0.0440)	(0.0726)	(0.0631)
Sample Mean	16.77	16.77	16.77	18.40	16.10
District FE	No	Yes	Yes	No	No
Sample	Full	Full	Full	$\geq$1 yr School	No School
Observations	19655	19655	19655	8924	10731

Notes: (1) Data from IHDS 2005.

(2) Robust standard errors in parentheses.

(3) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

first marriage on age of menarche and controls. Columns (1) and (2) show the results with and without district fixed effects. Column (3) shows the results using a non-linear functional form for age of menarche. There is a strong first stage across all these specifications. For the main specification with the full set of controls and district fixed effects (column 2), with every additional year that puberty is delayed, age of marriage increases by 0.47 years.

The second condition for identification of the IV model, the exclusion restriction, requires that age of menarche does not affect the outcomes of interest through any channel other than age of marriage. While most of the variation in age of menarche is random genetic variation, Field and Ambrus (2008) provide a discussion of environmental factors that may affect age at menarche.[3] In all the regressions I use district fixed effects and look for variation within districts, thus minimizing some of the variation in age of menarche from factors distributed geographically.

Very early and late puberty are linked to chronic medical conditions and extreme physical and emotional stress (Palmert and Boepple 2001). I therefore restrict the sample such that age of menarche ranges from 11 to 16, which covers 97 percent of the population. Of the environmental factors that affect age at menarche, the most relevant threat to identification is the correlation between nutrition and age at menarche. Nutrition may be related to family background and income, which may in turn directly affect adult outcomes. Columns (2) to (4) of Table 3.1 provide summary statistics for sample characteristics split by age of menarche. Acute malnutrition in utero or during childhood can delay the onset of puberty. Moreover such severe malnutrition is also associated with stunting. However, this does not seem to be the case comparing height across the different ages of menarche. Moreover,

[3]Field and Ambrus (2008) lists the following factors based on evidence from laboratory experiments: geography and climate, strenuous physical activity or stress, exposure to endocrine-disrupting chemicals, sex composition of peer group and abrupt changes in diet resulting in acute malnutrition in utero or during childhood.

the correlation between adult height and age at menarche in a regression is small and positive which is the opposite direction from what would be expected if malnutrition was driving the variation in age of menarche.[4] I include height as control in all the regressions.

A second variable malnutrition may be correlated with is the socio-economic status of the woman's natal family. However, since I do not observe detailed data on this, I consider an indicator for whether the woman belongs to a schedule caste or scheduled tribe household. SC/ST households are typically considered to be disadvantaged, which would suggest that women from SC/ST households are more likely to have suffered malnutrition leading to delayed onset of puberty. However, columns (2) to (4) in Table 3.1 indicate a trend in the opposite direction. The correlation between belonging to a SC/ST household and age of menarche is negative in a regression of age of menarche on these variables.[5] I also include this variable as a control.

3.4 Results

3.4.1 Education

I first look at the impact of age of marriage on education by measuring the effect on years of education completed and literacy. Table 3.3 shows the IV estimates using age of menarche as an instrument for age of marriage. The results show that delaying marriage by 1 year increases the years of education completed by the woman by 0.5 years and increases literacy by 4.3 percentage points. These estimates are comparable to the estimates found for the effect of delaying marriage on education for the sample of rural

[4]This is consistent with evidence from developmental biology that early puberty is associated with lower stature.

[5]In results not presented in this paper, I find that the results hold for both SC/ST and non-SC/ST households separately suggesting that the main results are not driven by the variation in age of menarche between SC/ST and non-SC/ST households.

Table 3.3: *Effect on Education*

	Years of Educ	Literacy
	(1)	(2)
Age of First Marriage	0.499***	0.0432***
	(0.0549)	(0.00684)
Age	-0.210***	-0.0270***
	(0.0234)	(0.00305)
Age Square	0.00147***	0.000228***
	(0.000350)	(0.0000459)
Height	0.0413***	0.00387***
	(0.00399)	(0.000508)
Muslim	-1.466***	-0.138***
	(0.0967)	(0.0131)
Christian	-0.244	-0.0317
	(0.201)	(0.0237)
SC/ST	-1.480***	-0.185***
	(0.0612)	(0.00788)
Constant	-6.579***	-0.401***
	(1.213)	(0.150)
Sample Mean	3.24	0.45
District Fixed Effects	Yes	Yes
Observations	19562	19613

Notes:

(1) Data from IHDS 2005

(2) Columns (1) and (2) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

women in Bangladesh (Ambrus and Field 2008).

3.4.2 Health, Autonomy and Mobility

Table 3.4 presents the IV estimates for the effect of age of marriage on health and use of contraception and health care. I find that age of marriage has a negative and insignificant effect on the woman's body mass index (BMI) which I use as a metric for her health status (column (1)). The BMI was constructed using the anthropometric measurements taken during the survey. Column (2) shows a decrease in the likelihood that the woman uses any type of contraception by about 2.2 percentage points. In columns (3) and (4) I present results on use of prenatal care and immunization among women who have had atleast one child born between year 2000 and the time of the survey. I find a small negative and insignificant effect of age of marriage on both these measures. These findings are in contrast to the effects found in Bangladesh, where women who marry later have fewer restrictions on consumption and mobility and show an increase in the use of prenatal care (Ambrus and Field 2008).

Table 3.5 presents the results for the effect of age of marriage on measures of autonomy. I find that increasing the age of marriage by one year decreases the likelihood of whether the woman has a job with a wage by 1.2 percentage points (column (1)). Increasing age of marriage by one year also decreases the likelihood that the woman visits her natal family more than once a year by 2.4 percentage points. Proximity and contact with natal family is typically associated with lower domestic violence and better outcomes for women. Columns (3) and (4) show the effect of age of marriage on an index of financial autonomy and mobility respectively. The financial autonomy index is a Z score index of the following variables: whether the woman has cash in hand for household expenditures, whether her name is on any bank account and whether her name is on home ownership or rental papers. The mobility index is a Z score index of the following

Table 3.4: *Effect on Health, Contraception and Health Care Usage*

	Body Mass Index	Using Contraception	Last birth: Antenatal Check Up	Last birth: Immunization
	(1)	(2)	(3)	(4)
Age of First Marriage	-0.121	-0.0217***	-0.000878	-0.00934
	(0.116)	(0.00719)	(0.00987)	(0.00919)
Age	0.0704	0.0824***	-0.00635	0.0294***
	(0.0633)	(0.00311)	(0.00736)	(0.00679)
Age Square	-0.000257	-0.00110***	-0.00000244	-0.000576***
	(0.000953)	(0.0000469)	(0.000123)	(0.000114)
Height	-0.286***	0.00108**	0.00363***	0.000746
	(0.0556)	(0.000488)	(0.000699)	(0.000626)
Muslim	-0.0571	-0.124***	-0.0460**	-0.109***
	(0.214)	(0.0127)	(0.0182)	(0.0175)
Christian	1.074*	0.0510*	0.0143	0.0620***
	(0.611)	(0.0275)	(0.0327)	(0.0227)
SC/ST	-0.924***	-0.0637***	-0.0753***	-0.0197*
	(0.166)	(0.00811)	(0.0117)	(0.0102)
Constant	65.23***	-0.880***	0.689***	0.845***
	(8.387)	(0.159)	(0.172)	(0.162)
Sample Mean	21.08	0.53	0.73	0.83
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	19647	17745	7272	7374

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) Questions on last birth (columns (1) & (2)) were only asked to women who had a birth after 2000.

(4) Robust standard errors in parentheses.

(5) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 3.5: *Effect on Autonomy*

	Working for Wage (1)	Visit Natal Family: >Once per Year (2)	Financial Autonomy Index (3)	Mobility Index (4)
Age of First Marriage	-0.0115*	-0.0237***	-0.102***	-0.122***
	(0.00616)	(0.00673)	(0.0274)	(0.0414)
Age	0.0406***	-0.0166***	0.0978***	0.394***
	(0.00271)	(0.00286)	(0.0116)	(0.0184)
Age Square	-0.000551***	0.000132***	-0.000980***	-0.00485***
	(0.0000412)	(0.0000439)	(0.000178)	(0.000276)
Height	-0.00119***	0.00146***	0.0112***	0.00669**
	(0.000413)	(0.000458)	(0.00188)	(0.00285)
Muslim	0.00170	0.0168	-0.258***	-0.330***
	(0.0106)	(0.0126)	(0.0489)	(0.0781)
Christian	0.00131	0.0149	-0.0686	0.212*
	(0.0231)	(0.0221)	(0.114)	(0.129)
SC/ST	0.186***	-0.0332***	-0.234***	0.237***
	(0.00744)	(0.00771)	(0.0298)	(0.0477)
Constant	-0.294**	1.485***	-2.564***	-5.455***
	(0.130)	(0.145)	(0.603)	(0.903)
Sample Mean	0.29	0.74	1.07	2.43
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	19655	19045	19655	19655

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) In columns (3) and (4) use a Z score index.

(4) Robust standard errors in parentheses.

(5) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

variables: whether the woman can go alone to a local health center, to the home of a relative or friend, to a kirana shop and whether she does the food and vegetable shopping for the household. The results indicate that women who are older at the time of marriage have lower mobility and financial autonomy. Tables C1 and C2 in the Appendix show the effects on the individual components of these indices.[6]

3.5 Mechanism

The results discussed in Section 3.5 suggest that women who are older at the time of marriage later do worse in terms of autonomy after they are married. We would typically expect education to be associated with improvements in these outcomes. Columns (1) and (2) of Table C3 in the Appendix show the results from a simple ordinary least squares regression of the outcome variables on years of education and age of marriage, respectively.[7] In the OLS, more education is correlated with more mobility and autonomy. However, age at the time of marriage can negatively affect these outcomes if men prefer marrying younger women. Women who are older may do worse in terms of marriage outcomes and marry worse spouses, especially if dowry does not compensate for undesirable traits. Amrbus and Field (2008) show that women who are older at the time of marriage in Bangladesh pay larger dowries and there are no significant differences in measures of spouse quality by age of marriage, suggesting that dowry compensates for a potentially lower value due to age in the marriage market.

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[6]Table C1 shows that increasing the age of first marriage by one year decreases the likelihood that the woman has cash in hand for household expenditures by 2.1 percentage points and the likelihood that her name is on home ownership or rental papers by 1.7 percentage points. There is a very small positive but insignificant effect on whether the woman's name is on any bank account. Further, Table A2 shows the effect on going alone to a local health center or the home of a relative or friend is small and negative, but insignificant. However, women who marry later are 2.6 percentage points less likely to be able to go alone to a kirana shop and 2.4 percentage points less likely to do the food and vegetable shopping for the household for every one year increase in age of marriage.

[7]The OLS regression includes the full set of controls and district fixed effects.

Table 3.6: *Effect on Marriage Market Outcomes*

	Spouse Years of Educ (1)	Educ Gap (2)	Spouse Agric Labor (3)	Monthly per Capita Consumption (4)	Natal Family Better Off (5)
Age of First Marriage	0.481***	-0.0122	-0.0200***	36.70***	0.0168***
	(0.0695)	(0.0642)	(0.00680)	(11.02)	(0.00645)
Age	-0.216***	-0.00140	0.0146***	-53.66***	0.00495*
	(0.0304)	(0.0282)	(0.00306)	(5.087)	(0.00265)
Age Square	0.00198***	0.000422	-0.000235***	0.928***	-0.0000595
	(0.000461)	(0.000425)	(0.0000462)	(0.0793)	(0.0000401)
Height	0.0579***	0.0147***	-0.00323***	4.417***	-0.000282
	(0.00537)	(0.00446)	(0.000472)	(0.826)	(0.000413)
Muslim	-1.790***	-0.423***	0.00483	-123.3***	0.00427
	(0.131)	(0.115)	(0.0113)	(16.62)	(0.0112)
Christian	-0.741***	-0.105	0.0668***	-7.317	-0.0558***
	(0.247)	(0.226)	(0.0219)	(61.58)	(0.0206)
SC/ST or OBC	-1.766***	-0.212**	0.114***	-200.5***	-0.00433
	(0.0930)	(0.0868)	(0.00854)	(15.40)	(0.00839)
Constant	-4.759***	1.915	0.724***	772.4***	0.113
	(1.608)	(1.444)	(0.148)	(278.5)	(0.144)
Sample Mean	5.57	2.32	0.28	731.88	0.18
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	18017	17962	18090	19643	18750

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (5) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

The data I use does not include detailed information on dowry for individual marriages, so I directly look at measures of spouse quality. Table 3.6 presents the results for effect of age of marriage on spouse's education, occupation, current monthly per capita income and relative economic status of the spouse's family compared to the woman's. Column (1) shows that woman who marry later are matched with spouses who are more educated. This is consistent with the fact that these women are more educated themselves, and that there are social norms that require husbands to have higher education levels than wives. Considering the effect on education gap between spouses in column (2) suggests that this type of positive assortative matching on education may be the case, since the effect on education gap is small and insignificant.

Column (3) shows that the spouses of women who marry later are also less likely to work in agricultural labor, which is consistent with the fact that their spouses have more education. Further, their households also have a higher current monthly per capita consumption, again consistent with the higher education of the spouse (column(4)). These results are contradictory to the results in Bangladesh where spouse quality did not differ by age of marriage. But rather than marry worse spouses, women who are older at the time of marriage get married to spouses of better quality. However, the results in column (5) shows that while they may be marrying spouses of better quality in terms of education, women who get married later consider their natal family to have been better off economically relative to their spouse's at the time of marriage. Thus, women who are older at the time of marriage marry into households that are of worse quality than their own, which might explain lower autonomy.

The positive effects on spouse quality may be due to more education of the women. Moreover, if the positive effects on spouse quality are driven by positive assortative matching, uneducated women who are older at the time of marriage would do worse on

Table 3.7: *Effect on Marriage Market Outcomes*

	Spouse Years of Educ (1)	Spouse Agric Labor (2)	Monthly per Capita Consumption (3)	Natal Family Better Off (4)
Age of Marriage X Illiterate	-0.233	-0.00711	-80.89***	-0.0542***
	(0.154)	(0.0157)	(29.92)	(0.0162)
Age of Marriage	0.447***	-0.00637	76.68***	0.0479***
	(0.114)	(0.0113)	(25.20)	(0.0129)
Illiterate	-0.0488	0.282	1170.8**	0.895***
	(2.598)	(0.265)	(508.6)	(0.274)
Age	-0.114***	0.00990***	-51.38***	0.00313
	(0.0282)	(0.00308)	(5.193)	(0.00281)
Age Square	0.00116***	-0.000190***	0.933***	-0.0000267
	(0.000428)	(0.0000464)	(0.0811)	(0.0000428)
Height	0.0384***	-0.00229***	3.039***	-0.000696
	(0.00471)	(0.000456)	(0.838)	(0.000427)
Muslim	-1.320***	0.00777	-87.79***	0.0149
	(0.122)	(0.0117)	(18.40)	(0.0123)
Christian	-0.275	0.0322	23.42	-0.0668***
	(0.222)	(0.0220)	(62.12)	(0.0222)
SC/ST	-1.000***	0.120***	-142.4***	-0.0138**
	(0.0685)	(0.00762)	(10.23)	(0.00681)
Constant	-0.826	0.316	349.0	-0.321
	(2.076)	(0.202)	(439.8)	(0.228)
Sample Mean	5.57	0.28	731.88	0.18
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	18001	18049	19601	18709

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

the marriage market. To test whether there is positive assortative matching by education, and whether education mediates some costs of delaying marriage, I look at differences in marriage outcomes by literacy. In the sample, 55 percent of the population is not literate and for these women delaying marriage may be more costly. Table 3.7 presents the results for the effect of age of marriage interacted with whether the woman is illiterate on marriage outcomes.[8] Column (1) shows that while age of marriage increases spouse education for literate women, it decreases spouse education for illiterate women (slope is negative but not significant at the 10 percent level). Similarly, in the long run, monthly per capita consumption declines with age of marriage for illiterate women, while it increases with age of marriage for literate women (column (3)). These results suggest that education mediates some costs of age of marriage and delaying marriage is even more costly for uneducated women since these women do not benefit from the positive assortative matching from higher education. Tables C5 and C6 in the Appendix show the effect of age of marriage interacted with whether the woman is illiterate on health and autonomy respectively. Illiterate women do worse than literate women in some outcomes, especially use of health care and whether they are working. However the benefit for literate women is not consistent, particularly among the autonomy outcomes.

Worse spouse quality can explain the worse later-life outcomes for uneducated women. However, among educated women, the difference in the spouse's family may be an important factor. Even though the spouse has higher education, as age of marriage increases, the spouse is more likely to come from a family that is economically worse off than the woman's (column (4) of Table 3.7). Cultural values and beliefs persist and are slow to change (Fernandez and Fogli 2009). In a context where less than 30 percent of women work, and most couples co-reside with their husband's parents, household values about

[8]I use whether the woman is illiterate rather than years of education here because years of education moves with age of menarche. The illiterate women in the sample have never been to school and the decision to not send the girl to school was made before any signs of puberty appear.

female mobility and autonomy may depend on the initial economic status of the spouse's family. This could explain why women who are older at the time of marriage still do worse in terms of later-life outcomes (like female autonomy) that are more likely to be dependent on culture and beliefs. Table C4 in the Appendix shows the woman's beliefs on which situations it is common for husbands to beat their wives in their community. Women who marry later are more likely to state that in their communities women who go outside without telling their husbands are likely to be beaten (column (1)).[9] This suggests that there are more restrictions on female mobility in their communities, which may be reflective of their own limited mobility.[10]

These results suggest that education has some benefits on the marriage and women who are older get married to men who are more educated because the woman herself is more educated. But these men are from families that are worse off than the woman's because the woman has a lower value in the marriage market because she is older. Moreover, because of the higher education, their spouses are less likely to work in agricultural labor, and they do better economically in the long run in terms of current monthly per capita consumption.

3.6 Conclusion

This paper studies the effect of delaying marriage on later-life outcomes for women in rural India. I find that women who marry later get more education, but do worse in terms of later-life outcomes related to mobility and autonomy. I find evidence that education has some benefits on the marriage market through positive assortative matching.

[9]The survey asks the women about domestic violence in their community and not in their family.

[10]The results in Table C4 in the Appendix also show that women who are married later believe that husbands in their community beat their wives if they suspect their wives of having an extramarital affair. However, they are less likely to believe that husbands beat their wives if they do not get the expected dowry from the natal family which might be reflective of the fact that dowry is not legal in India.

However, the costs to delaying marriage may mean that women who marry later marry spouses from families that are economically worse off than their own. In a setting where most couples co-reside with the spouse's family, if values on gender roles persist across generations, this could be a plausible explanation for why women who marry later do worse in some measures of later-life outcomes that are more likely to be determined by such beliefs. Further research is required to explore this mechanism.

When women delay marriage due to employment opportunities, this leads to increases in female empowerment and autonomy (Sivasankaran 2014). However, while age of menarche provides a biological lower bound on age of first marriage, such a delay to marriage is costly for female autonomy. While there is evidence that women who marry later get more years of schooling, education by itself does not seem sufficient to change gender roles in the household. The primary benefit of education in this setting is that it increases the education of the spouse. The difference between working and schooling before marriage may be that working outside the household changes the exposure and bargaining power that the woman has. On the other hand, with schooling, women remain in their villages and live at home. Moreover, work experience and income may be able to compensate for being older at the time of marriage, while educational attainment may not be valued in this context. Finally, while a larger dowry may be able to overcome the costs of delaying marriage in some settings (Ambrus and Field 2008), this may not work in other marriage markets. The findings in this paper suggest that while preventing early marriage is an important policy goal, it is important to consider the channel through which the delay occurs and the conditions in the local marriage market.

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Appendix A

Appendix to Chapter 1

A.1 Tracking Methodology

I implemented a multi-step tracking process using a team of field staff from the Center for MicroFinace to identify the location of the workers for the survey and ensure minimal attrition from the sample. We piloted the process in one district first to assess the tracking success before expanding to the rest of Tamil Nadu. 130 surveys were conducted in the pilot round. The process involved the following stages:

A.1.1 Stage 1 - Firm Contact Data

We first used the contact data obtained from the firm to extract information on the district and *taluk* (the next sub-division below district) the worker was originally from, whenever this information was available. We then created smaller lists grouped by region and date of joining. Grouping them by region and date of joining meant we could then also rely on worker networks to improve our ability to find workers.

A.1.2 Stage 2 - Tracking by phone

Following this, we contacted all workers from the list who had provided a phone number in their contact information. For the workers whom we were able to contact successfully, we verified and updated addresses with the most recent contact information. Further, if the contact information belonged to a family member, we collected the current contact information of the worker from this person.

A.1.3 Stage 3 - In-person tracking

We then conducted in-person visits to verify and update contact information. The addresses were organized by area and each area was visited by a member of the tracking team. If the worker had migrated from the area for marriage, family members we asked for the worker's current contact information. We also asked workers for contact information of other workers who had worked at the same time as them for whom we did not have proper contact information. In cases in which the worker was not available for the survey, we requested an immediate family member to participate in the survey and answer the main sections of the survey.

We ran several iterations of this process in each district until we had attempted to track all the workers on the list. As the tracking process was completed in each district, a separate survey team of only female surveyors visited each worker (or family member) to conduct the follow-up survey. We used cell phones and tablet devices to do electronic surveys so that we could monitor and assign work to the tracking and survey teams in real time. For about 17 percent of the surveys, a family member responded to the questions. In such surveys, we dropped questions on attitudes and only asked questions that measured real outcomes.[1]) In Table A1, I provide the final results from the tracking

[1]There are no significant differences by exposure to the fixed-term contract on whether the survey was given by a family member.

process we used. I first show the tracking results for the full sample of all workers hired since 2007. I then show the tracking results for only those workers who were working at the firm when the change in policy was implemented. Our tracking process was able to successfully track and complete surveys for about 70 percent of the sample. About 10 percent of the sample refused to participate in the survey and about 20 percent could not be found through the tracking process.

Table A2 shows the OLS estimates regressing the tracking outcomes on the cohort of joining and type of contract. The results show that the probability of completing the survey successfully is about 15 percent lower for the cohort that joined the firm 24 to 30 months before the change in wage policy by the firm. All the analysis in the paper is done for the full sample and a restricted sample defined as the sample dropping this cohort. Table A3 shows that there is no difference in tracking by the instrumental variable. I present the results for the full sample as well as the restricted sample are presented.

A.2 Description of Indices

A.2.1 Spouse Quality Index

This index consists of the following 5 variables that measure spouse quality.

(1) Age gap between the worker and her spouse

Lower age gap indicates better quality.

(2) Spouse‘s education relative to the worker‘s education

More education indicates better quality.

(3) Spouse and his family‘s economic status relative to worker‘s

Better economic status indicates better quality.

(4) Spouse's income

Higher income indicates better quality.

(5) Whether the spouse is from the same district

Being from the same district indicates better quality since marrying further away is associated with worse outcomes such as domestic violence (Fulford 2013)

Panel A of Table A4 shows the IV results for the impact of working on the individual components of this index.

A.2.2 Empowerment Index

This index consisted of 12 statements about gender roles that were posed to the respondents. For each statement, the respondent was expected to give one of 2 answers: Agree with statement, or Disagree with statement, with each coded separately for which answer indicated empowerment.

(1) I feel safe to walk/move in my village/area alone during the day.

Agreement indicates empowerment.

(2) A girl should be allowed to study as much as she wants.

Agreement indicates empowerment.

(3) Women should not work outside home after they get married.

Disagreement indicates empowerment.

(4) It is unsafe for an adolescent girl to go outside of her home alone.

Disagreement indicates empowerment.

(5) A husband should earn more money than his wife.

Disagreement indicates empowerment.

(6) Girls should not be allowed to engage in income generating activities that require them to go outside the house.

Disagreement indicates empowerment.

(7) For the most part, it is better to be a man than to be a woman.

Disagreement indicates empowerment.

(8) Girls should get married as soon as they leave school.

Disagreement indicates empowerment.

(9) Girls should be allowed to wear whatever they want without being harassed.

Agreement indicates empowerment.

(10) A husband should be more educated than his wife.

Disagreement indicates empowerment.

(11) Parents should maintain stricter control over their daughters than their sons.

Disagreement indicates empowerment.

(12) I would prefer sons to daughters.

Disagreement indicates empowerment.

Figure A2 shows the distribution of the aggregated Z scores of the components of this index.

A.2.3 Internal Locus of Control

This index consisted of 5 statements about the ability to control outcomes in one's life that were posed to the respondents. A high internal locus of control indicates belief that events in one's life can be affected by one's actions rather than outside factors. For each statement, the respondents were asked to choose one of the following: Strongly Agree,

Agree, Neither agree or disagree, Disagree, Strongly Disagree. While coding however, Strongly Agree and Agree were mapped to the same Agree value, while Disagree and Strongly Disagree were both coded to a generic Disagree.

(1) There is no real way that I can solve the problems I have.

Disagreement indicates a high internal locus of control.

(2) People's misfortunes result from the mistakes they make.

Agreement indicates a high internal locus of control.

(3) I have little control over the things that happen to me.

Disagreement indicates a high internal locus of control.

(4) Many of the unhappy things in people's lives are partly due to bad luck.

Disagreement indicates a high internal locus of control.

(5) There is little I can do to change many of the important things in my life.

Disagreement indicates a high internal locus of control.

Figure A3 shows the distribution of the total number of responses to the above statements that indicate that the survey respondent has an internal locus of control.

A.2.4 Marriage Decisions and Attitudes

This index posed 2 questions about marriage decisions to the respondents. Each answer was indicative of more or less empowerment for that respondent.

(1) What is the earliest age you would have wanted to getting married? *Higher the age, higher the level of empowerment*

(2) Do you think you will be allowed to refuse marriage proposal? *Answering "Yes" to this question is indicative of higher empowerment*

Panel B of Table A4 shows the IV results for the impact of working on the individual components of this index.

A.2.5 Autonomy in Work Decisions

This index posed 2 questions about the independence of job-related decisions taken by the respondents. Each answer was indicative of more or less empowerment for that respondent.

(1) Why did you stop working? (if respondent has stopped working) *Any answer that implies that the respondent stopped working because of the wishes of her parents, partners or other family members indicates lower empowerment*

(2) Who do you think should have control over the money you earn? *Any answer that implies that someone other than the respondent should have control over the money earned by the respondent indicates lower empowerment.*

Panel C of Table A4 shows the IV results for the impact of working on the individual components of this index.

A.3 Robustness Check: Results for Restricted Sample

In Tables A5 to A9, I present the main results for the restricted sample which omits the cohort that joined between 24 to 30 months before the change in the wage contract. The tracking results show that this cohort had a lower tracking rate. I find that the results are consistent with omitting this group.

A.4 Supplementary Tables and Figures

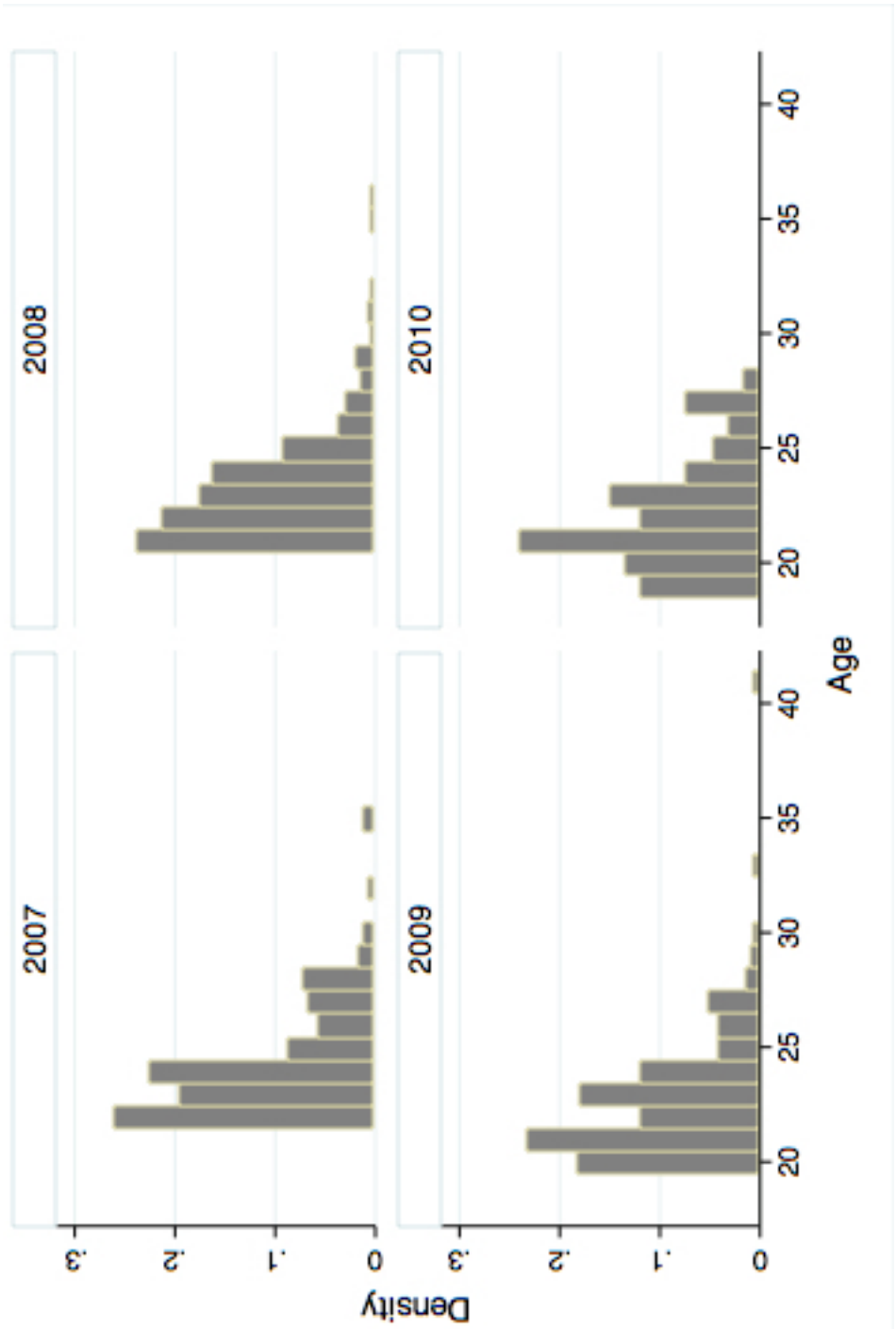

Figure A.1: *Distribution of Age by Year of Joining*

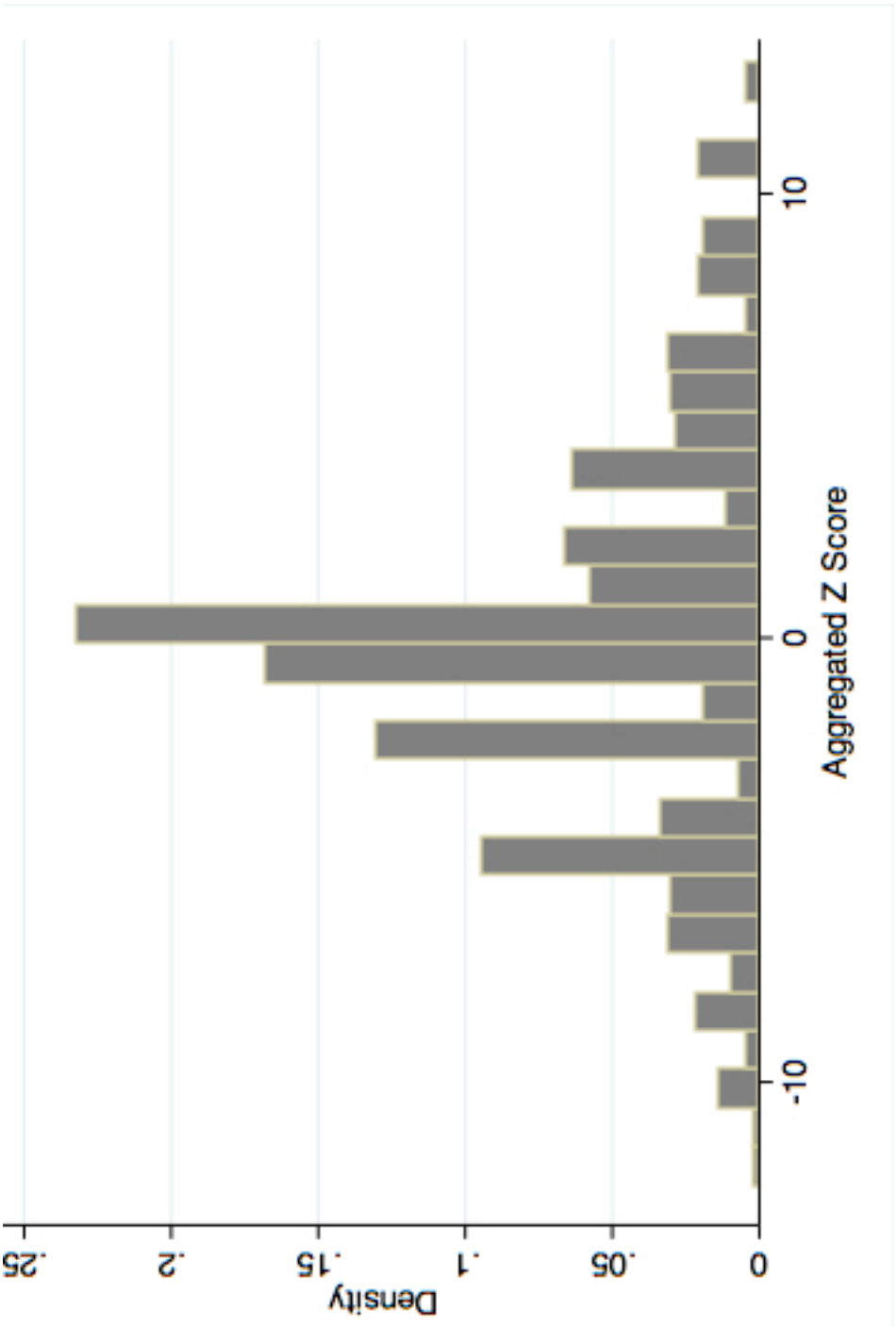

Figure A.2: *Components of Empowerment Index*

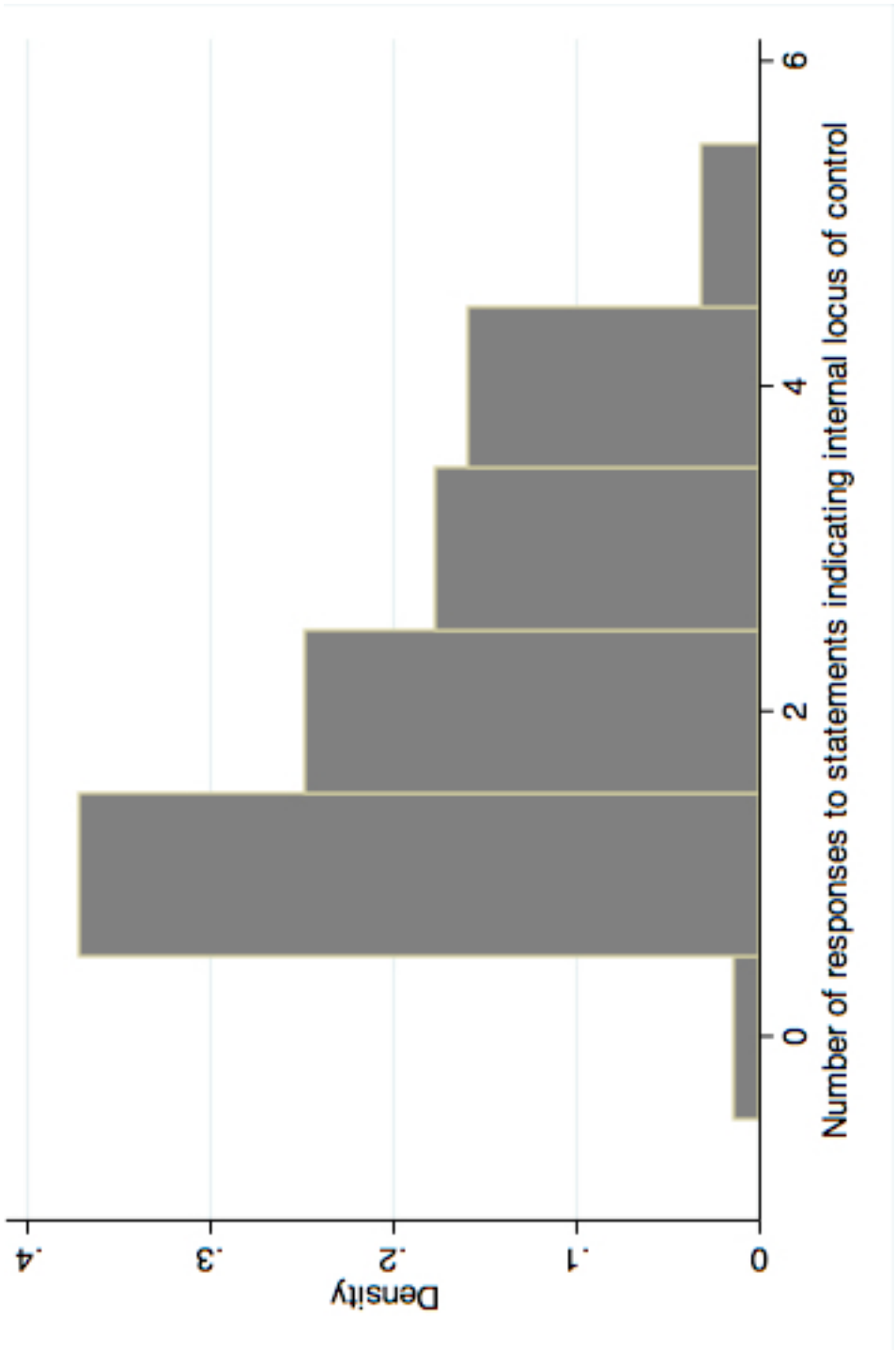

Figure A.3: *Components of Locus of Control Index*

Table A1: *Tracking Results*

Tracking Status	Number	Percent
Panel A: Full Sample (n=1414)		
Survey Complete	969	68.53
Refused/Cannot survey	122	8.63
Not Found	323	22.84
Panel B: Sample working at time of policy change (n=616)		
Survey Complete	435	70.62
Refused/Cannot survey	62	10.06
Not Found	119	19.32

Table A2: *Tracking by Cohort of Joining*

	Survey Completed (1)	Refused/ Cannot survey (2)
Join less than 6 months before	0.00201	0.0332
	(0.0471)	(0.0294)
Join 6 to 12 months before	0.0577	0.0258
	(0.0398)	(0.0241)
Join 12 to 18 months before	-0.00640	0.0379
	(0.0422)	(0.0250)
Join 18 to 24 months before	0.0564	0.00758
	(0.0362)	(0.0203)
Join 24 to 30 months before	-0.152***	0.00816
	(0.0496)	(0.0256)
3 year contract	0.00987	-0.0335*
	(0.0302)	(0.0194)
Constant	0.840***	0.150*
	(0.165)	(0.0880)
Observations	1409	1409

Notes:

(1) Columns (1) and (2) show the tracking by cohort of joining.

(2) Robust standard errors in parentheses

(3) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table A3: *Balance Check for Tracking*

	Survey completed		Refused/Cannot survey	
	(1)	(2)	(3)	(4)
Months before X 3 year	0.00105	0.00227	0.000267	0.000500
	(0.00249)	(0.00257)	(0.00145)	(0.00148)
Sample	Full	Reduced	Full	Reduced
Observations	1414	1273	1414	1273

Notes:

(1) Columns (1) and (4) show the effect of the change in wage policy on tracking results.

(2) Cohort of joining controls included in all specifications.

(3) Robust standard errors in parentheses

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A4: *Breakdown of Index*

Panel A: Spousal Quality Index (Sample: Married Workers)					
	Age Gap: Worker and Spouse	Spouse; Better Educ	Spouse family Better Econ	Log Spouse Income	Spouse; Inside District
	(1)	(2)	(3)	(4)	(5)
Months worked in factory	0.0174	-0.00286	-0.00962	0.000382	0.00254
	(0.0749)	(0.0102)	(0.00948)	(0.0141)	(0.00825)
	[0.816]	[0.779]	[0.310]	[0.978]	[0.758]
Sample Mean	6.038	0.328	0.255	8.666	0.797
Observations	598	591	597	587	598
Panel B: Marriage Decisions and Attitudes					
	Earliest age get married	Allowed to refuse proposal			
	(1)	(2)			
Months worked in factory	0.0845***	0.0113*			
	(0.0304)	(0.00636)			
	[0.005]	[0.076]			
Sample Mean	21.97	0.336			
Observations	686	690			
Panel C: Autonomy in Work Decisions					
	Stop Work Not Because Parents/Spouse Stopped	Earnings Control Not Husband			
	(1)	(2)			
Months worked in factory	0.00818**	0.0101			
	(0.00397)	(0.00657)			
	[0.039]	[0.125]			
Sample Mean	0.920	0.747			
Observations	750	826			

Notes: Standard errors in parentheses and *p*-values in brackets. Asterix indicate significance: * p<0.10, ** p<0.05, *** p<0.01

Table A5: *Age of Marriage*

	Married before age 21 (1)	Age of Marriage (2)	Age received first proposal (3)	Time between first proposal & marriage (4)
Panel A: IV Results				
Months worked in factory	-0.00725	0.0772**	0.0514*	0.0333
	(0.00546)	(0.0388)	(0.0275)	(0.0352)
	[0.184]	[0.047]	[0.061]	[0.345]
Panel B: Reduced Form Results				
Months before X 3 year	-0.00378	0.0324**	0.0276*	0.0145
	(0.00291)	(0.0161)	(0.0148)	(0.0153)
	[0.194]	[0.044]	[0.064]	[0.344]
Sample Mean	0.320	20.58	19.87	1.042
Observations	877	551	769	469

Notes:
(1) Table shows results for the restricted sample that drops the cohort that had a poor tracking rate in the survey
(2) Columns (1) to (4) of Panel A show the IV results for the impact of working outside the household on age of marriage.
(3) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(4) Columns (1) to (4) of Panel B show the reduced form results for the effect of duration under the old contract on age of marriage.
(5) Individual controls for age and education and cohort of joining fixed effects included.
(6) Results are consistent with dropping cohort of joining fixed effects.
(7) About 40% of the sample is unmarried and hence the sample size in columns (1) and (4) is smaller.
(8) Robust standard errors in parentheses and *p*-values in brackets.
(9) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table A6: *Costs of Delaying Marriage*

	No. of marriage proposals received (1)	Ever Married (2)	Log Gifts given during Wedding (3)	Spouse Quality Index (4)
Panel A: IV Results				
Months worked in factory	-0.0144	-0.000472	0.0118	-.0055
	(0.0474)	(0.00581)	(0.0745)	(0.0094)
	[0.761]	[0.935]	[0.875]	[0.560]
Panel B: Reduced Form Results				
Months before X 3 year	-0.00756	-0.000228	0.00473	-0.00216
	(0.0253)	(0.00285)	(0.0306)	(0.00395)
	[0.765]	[0.936]	[0.878]	[0.585]
Sample Mean	2.980	0.610	181242.9	.
Observations	784	908	543	.

Notes:
(1) Table shows results for the restricted sample that drops the cohort with a poor tracking rate.
(2) Columns (1) to (3) of Panel A show the IV results for the impact of working outside the household on the marriage market.
(3) The change in wage policy is used as an instrumental variable for months worked in the factory.
(4) Columns (1) to (3) of Panel B show the reduced form results for the effect of duration under the old contract on the marriage market.
(5) Columns (4) shows the average effect size for the impact of working outside the household on spousal quality and the effects can be interpreted as standard deviation changes.
(6) Please refer to the appendix section 2 for the composition of the index.
(7) Individual controls for age and education and cohort of joining fixed effects included.
(8) Results are consistent with dropping cohort of joining fixed effects.
(9) About 40% of the sample is unmarried and hence the sample size in column (3) is smaller.
(10) Robust standard errors in parentheses and *p*-values in brackets.
(11) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A7: *Fertility*

	Child before age 23 (1)	Age when first child was born (2)	No. of kids currently (if married) (3)	No. of kids currently (full sample) (4)	Desired Fertility (5)
Panel A: IV Results					
Months worked in factory	-0.0124**	0.0849*	-0.0217	-0.0232**	-0.0172**
	(0.00606)	(0.0511)	(0.0171)	(0.0106)	(0.00832)
	[0.041]	[0.097]	[0.203]	[0.028]	[0.038]
Panel B: Reduced Form Results					
Months before X 3 year	-0.00618**	0.0282*	-0.00835	-0.0114**	-0.00859**
	(0.00309)	(0.0169)	(0.00700)	(0.00535)	(0.00389)
	[0.046]	[0.097]	[0.233]	[0.033]	[0.028]
Sample Mean	0.362	21.53	0.946	0.516	1.928
Observations	625	336	424	779	526

Notes:
(1) Table shows results for the restricted sample that drops the cohort that had a poor tracking rate in the survey
(2) Columns (1) to (5) of Panel A show the IV results for the impact of working outside the household on fertility.
(3) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(4) Columns (1) to (5) of Panel B show the reduced form results for the effect of duration under the old contract on fertility.
(5) Individual controls for age and education and cohort of joining fixed effects included.
(6) Results are consistent with dropping cohort of joining fixed effects.
(7) About 40% of the sample is unmarried and in the pilot round of the survey we did not ask the number of children the woman had; therefore we do not have the full sample in columns (1) to (3).
(8) Desired fertility was added in a later version of the survey and hence has a smaller number of observations.
(9) Robust standard errors in parentheses and *p*-values in brackets.
(10) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table A8: *Empowerment and Autonomy*

	Empowerment	Internal Locus of Control	Marriage Decisions & Attitudes	Autonomy in Work Decisions
	(1)	(2)	(3)	(4)
Panel A: IV Results				
Months worked in factory	0.0070*	0.0122**	0.0333***	0.0264**
	(0.0039)	(0.0061)	(0.0107)	(0.0118)
	[0.074]	[0.044]	[0.002]	[0.025]
Panel B: Reduced Form Results				
Months before X 3 year	- 0.00400	0.00698*	0.0187***	0.0130**
	(0.00293)	(0.00375)	(0.00611)	(0.00567)
	[0.172]	[0.062]	[0.002]	[0.022]

Notes:
(1) Table shows results for the restricted sample that drops the cohort that had a poor tracking rate in the survey.
(2) Columns (1) to (4) show the average effect size for the impact of working outside the household on empowerment and autonomy and the effects can be interpreted as standard deviation changes..
(3) Please refer to the appendix section 2 for the composition of the index.
(4) The change in wage policy by the firm is used as an instrumental variable for months worked in the factory.
(5) Panel A and B show the IV and reduced form results respectively.
(6) Individual controls for age and education and cohort of joining fixed effects included.
(7) Results are consistent with dropping cohort of joining fixed effects.
(8) Robust standard errors in parentheses and *p*-values in brackets.
(9) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table A9: *Household Wealth*

	Log Household Income	Savings	Loans	HouseHold Assets
	(1)	(2)	(3)	(4)
Panel A: IV Results				
Months worked in factory	0.0130	-2632.4	217.6	0.0328
	(0.00958)	(2132.5)	(1894.4)	(0.0323)
	[0.174]	[0.217]	[0.909]	[0.310]
Panel B: Reduced Form Results				
Months before X 3 year	0.00629	-1244.4	112.9	0.0160
	(0.00457)	(1017.6)	(1008.3)	(0.0159)
	[0.169]	[0.222]	[0.911]	[0.315]
Sample Mean	4907.3	58245.6	26870.4	5.722
Observations	884	648	554	908

Notes:
(1) Table shows results for the restricted sample that drops the cohort that had a poor tracking rate in the survey
(2) Columns (1) to (4) of Panel A show the IV results for the impact of working outside the household on household wealth.
(3) The change in wage policy by the firm is used as an instrumental variable for months worked in factory.
(4) Columns (1) to (4) of Panel B show the reduced form results for the effect of duration under the old contract on on household wealth.
(5) Individual controls for age and education and cohort of joining fixed effects included.
(6) Results are consistent with dropping cohort of joining fixed effects.
(7) Some respondents were not able to provide the value of savings and loans and hence the sample size in columns (2) and (3) is smaller.
(8) Robust standard errors in parentheses and *p*-values in brackets.
(9) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Appendix B

Appendix to Chapter 2

B.1 Supplementary Tables

Table B1

Number of Days Spent by the Household in different activities in the last 7 days:

The dependent variable is total days worked in each activity Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) All members (Age 10 to 60)	(2) Adults (Age 18 to 60)	(3) Children (Age 10 to 17)
Panel A: Total Days Working Outside			
NREG	0.187*	0.153	0.079*
	(0.114)	(0.107)	(0.044)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	4.336	4.064	0.560
Panel B: Total Days Working in HH Enterprise			
NREG	-0.268*	-0.247*	-0.039
	(0.142)	(0.129)	(0.062)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	6.767	6.402	0.753
Panel C: Total Days in Domestic work			
NREG	-0.046	-0.085	0.079
	(0.098)	(0.090)	(0.074)
Observations	229,506	229,506	110,637
non-NREG mean of dependent variable	6.081	5.498	1.201

Note: Includes controls for household size, religion and social group. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B2

Number of Days Spent on Different Activities in the last 7 days:

Boys: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.231**	-0.167*	0.074*	0.083
	(0.093)	(0.093)	(0.044)	(0.082)
Observations	36,714	36,714	36,714	36,714
Non-NREG mean	0.959	1.104	0.129	0.745
Panel B: Age 10 to 14				
NREG	0.017	0.025	0.028	-0.019
	(0.024)	(0.035)	(0.025)	(0.055)
Observations	67,945	67,945	67,945	67,945
Non-NREG mean	0.141	0.208	0.092	0.542
Panel C: Age 6 to 9				
NREG	-0.007*	0.004	0.004	-0.201***
	(0.004)	(0.016)	(0.015)	(0.075)
Observations	52,724	52,724	52,724	52,724
Non-NREG mean	0.004	0.014	0.031	0.936

Note: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B3

Number of Days Spent by Girls on Different Activities in the last 7 days:

Girls: Age 6 to 17 years (Never married)

Includes District Fixed Effects and Year*Quarter Fixed Effects

	(1) Outside Work	(2) HH Enterprise	(3) Domestic Work	(4) Other
Panel A: Age 15 to 17				
NREG	0.048	0.030	0.047	0.064
	(0.057)	(0.076)	(0.128)	(0.050)
Observations	29,982	29,982	29,982	29,982
Non-NREG mean	0.429	0.606	2.315	0.308
Panel B: Age 10 to 14				
NREG	0.001	0.039	0.025	-0.096**
	(0.020)	(0.055)	(0.071)	(0.044)
Observations	59,421	59,421	59,421	59,421
Non-NREG mean	0.105	0.183	0.828	0.466
Panel C: Age 6 to 9				
NREG	-0.001	-0.003	0.036	-0.196**
	(0.004)	(0.012)	(0.034)	(0.083)
Observations	47,698	47,698	47,698	47,698
Non-NREG mean	0.004	0.014	0.083	1.179

Note: Includes controls for age, age^2, literacy, household size, religion and caste. Standard errors adjusted for clustering at 570 districts in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix C

Appendix to Chapter 3

C.1 Supplementary Tables

Table C1: *Effect on Financial Autonomy*

	Cash in Hand for HH Exp	Name on Any Bank Account	Name on Home Ownership/ Rental Papers
	(1)	(2)	(3)
Age of First Marriage	-0.0207***	0.00761	-0.0174***
	(0.00600)	(0.00561)	(0.00530)
Age	0.0248***	0.0106***	0.00383*
	(0.00270)	(0.00230)	(0.00219)
Age Square	-0.000312***	-0.000102***	0.0000230
	(0.0000400)	(0.0000357)	(0.0000343)
Height	0.00180***	0.00156***	0.000659*
	(0.000410)	(0.000399)	(0.000347)
Muslim	-0.0435***	-0.0430***	-0.0153*
	(0.0112)	(0.0102)	(0.00895)
Christian	-0.0144	-0.0258	0.00283
	(0.0238)	(0.0249)	(0.0224)
SC/ST	-0.0301***	-0.0466***	-0.0151***
	(0.00665)	(0.00604)	(0.00584)
Constant	0.423***	-0.440***	0.0868
	(0.136)	(0.124)	(0.111)
Sample Mean	0.80	0.14	0.15
District Fixed Effects	Yes	Yes	Yes
Observations	19616	17575	18839

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (3) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table C2: *Effect on Mobility*

	Go Alone: Local Health Center	Go Alone: Rel/Friend Home	Go Alone: Kirana Shop	Do Food/ Veg Shopping
	(1)	(2)	(3)	(4)
Age of First Marriage	-0.00153	-0.00529	-0.0261***	-0.0237***
	(0.00687)	(0.00706)	(0.00759)	(0.00682)
Age	0.0563***	0.0477***	0.0421***	0.0482***
	(0.00298)	(0.00299)	(0.00318)	(0.00294)
Age Square	-0.000705***	-0.000582***	-0.000527***	-0.000580***
	(0.0000446)	(0.0000445)	(0.0000473)	(0.0000445)
Height	0.000455	0.000875*	0.000840*	0.000805*
	(0.000473)	(0.000482)	(0.000505)	(0.000453)
Muslim	-0.0686***	-0.0179	-0.0404***	-0.0335***
	(0.0132)	(0.0127)	(0.0139)	(0.0126)
Christian	0.0148	-0.00833	0.0316	0.0608**
	(0.0213)	(0.0206)	(0.0212)	(0.0250)
SC/ST	0.0220***	0.0196**	0.0269***	0.0475***
	(0.00781)	(0.00778)	(0.00802)	(0.00780)
Constant	-0.245	-0.141	0.448***	-0.0208
	(0.151)	(0.154)	(0.165)	(0.152)
Sample Mean	0.65	0.69	0.73	0.53
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	19044	18685	16408	19548

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table C3: *OLS Regression: Coefficient on Age of marriage and Education*

Variable	Years of Education	Age of Marriage
	(1)	(2)
Body Mass Index	0.118***	0.0511**
	(0.0175)	(0.0224)
Using Contraception	0.00750***	-0.00953***
	(0.000954)	(0.00127)
Last Birth: Antenatal checkup	0.0151***	0.00591***
	(0.00121)	(0.00184)
Last Birth: Any immunization	0.00688***	0.00391**
	(0.00106)	(0.00165)
Working for Wage	-0.0189***	-0.00211*
	(0.000851)	(0.00111)
Visits Natal Family: >1 per year	0.00551***	0.00123
	(0.000852)	(0.00115)
Cash in Hand for HH exp	0.00698***	-0.000666
	(0.000729)	(0.000993)
Name of Any Bank Account	0.0171***	0.000185
	(0.000812)	(0.000912)
Name on home ownership/rental papers	0.00215***	-0.00191**
	(0.000703)	(0.000883)
Go Alone: Local Health Center	0.00528***	-0.00518***
	(0.000891)	(0.00121)
Go Alone: Relative/Friend's Home	0.00288***	-0.00486***
	(0.000900)	(0.00119)
Go Alone: Kirana Shop	0.00122	-0.00165
	(0.000943)	(0.00123)
Do Food/Veg Shopping	-0.00561***	-0.00464***
	(0.000895)	(0.00119)

Notes:
(1) Data from IHDS 2005
(2) Columns (1) and (2) report the coefficient from a regression of the outcome variables listed on education and age of marriage.
(2) The regression includes the full set of other controls and district fixed effects.
(3) Robust standard errors in parentheses.
(4) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table C4: *Values in Community on Domestic Violence*

	Goes Out Without Permission (1)	No Expected Dowry From Natal Family (2)	Neglects House or Children (3)	Doesn't Cook Properly (4)	Suspect Extramarital Affair (5)
Age of First Marriage	0.0352***	-0.0199***	-0.000201	-0.00632	0.0291***
	(0.00722)	(0.00676)	(0.00676)	(0.00657)	(0.00478)
Age	-0.00463	-0.0000925	0.000135	-0.000309	-0.00122
	(0.00308)	(0.00285)	(0.00286)	(0.00273)	(0.00192)
Age Square	0.0000555	-0.0000189	-0.0000227	-0.00000956	0.0000315
	(0.0000463)	(0.0000428)	(0.0000430)	(0.0000410)	(0.0000293)
Height	-0.00174***	0.000190	-0.000798*	-0.00117***	-0.000589*
	(0.000489)	(0.000416)	(0.000448)	(0.000426)	(0.000308)
Muslim	0.0724***	0.00820	0.0386***	0.0347***	0.0272***
	(0.0133)	(0.0124)	(0.0122)	(0.0117)	(0.00868)
Christian	-0.0696***	0.0115	-0.0619**	0.00193	-0.0359
	(0.0224)	(0.0236)	(0.0253)	(0.0211)	(0.0234)
SC/ST	0.0701***	0.0162**	0.0373***	0.0363***	0.0257***
	(0.00803)	(0.00751)	(0.00750)	(0.00713)	(0.00509)
Constant	0.448***	0.563***	1.031***	0.792***	0.540***
	(0.159)	(0.146)	(0.142)	(0.146)	(0.0962)
Sample Mean	0.42	0.28	0.35	0.30	0.86
District Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	19598	19597	19612	19618	19573

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (5) show IV results using age of menarche as an instrument for age of first marriage.

(3) Robust standard errors in parentheses.

(4) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01

Table C5: *Effect on Health, Contraception and Health Care Usage*

	Body Mass Index	Using Contraception	Last birth: Antenatal Check Up	Last birth: Immunization
	(1)	(2)	(3)	(4)
Age of Marriage X Illiterate	-0.301	0.0292*	-0.0491**	-0.0207
	(0.293)	(0.0174)	(0.0226)	(0.0203)
Age of Marriage	0.0175	-0.0423***	0.0296	0.00274
	(0.206)	(0.0133)	(0.0194)	(0.0175)
Illiterate	4.074	-0.555*	0.716*	0.285
	(4.936)	(0.293)	(0.383)	(0.344)
Age	0.0828	0.0851***	-0.0105	0.0281***
	(0.0656)	(0.00324)	(0.00849)	(0.00774)
Age Square	-0.000244	-0.00113***	0.0000844	-0.000542***
	(0.000991)	(0.0000489)	(0.000137)	(0.000126)
Height	-0.291***	0.000981**	0.00273***	0.000280
	(0.0557)	(0.000493)	(0.000695)	(0.000633)
Muslim	0.161	-0.123***	-0.0161	-0.0944***
	(0.219)	(0.0135)	(0.0195)	(0.0188)
Christian	1.040*	0.0608**	0.0136	0.0623***
	(0.604)	(0.0282)	(0.0328)	(0.0236)
SC/ST	-0.719***	-0.0540***	-0.0482***	-0.00513
	(0.167)	(0.00786)	(0.0112)	(0.00976)
Constant	63.98***	-0.519**	0.407	0.753***
	(8.842)	(0.235)	(0.283)	(0.255)
Sample Mean	21.08	0.53	0.73	0.83
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	19605	17714	7255	7358

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) Questions on last birth (columns (1) & (2)) were only asked to women who had a birth after 2000.

(4) Robust standard errors in parentheses.

(5) Asterisks denote significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table C6: *Effect on Autonomy*

	Working for Wage (1)	Visit Natal Family: >Once per Year (2)	Financial Autonomy Index (3)	Mobility Index (4)
Age of Marriage X Illiterate	-0.0433***	-0.0294*	-0.111	0.109
	(0.0156)	(0.0172)	(0.0701)	(0.105)
Age of Marriage	0.0211*	-0.00883	-0.0546	-0.192**
	(0.0115)	(0.0124)	(0.0549)	(0.0781)
Illiterate	0.881***	0.432	1.376	-1.958
	(0.263)	(0.290)	(1.187)	(1.771)
Age	0.0349***	-0.0161***	0.107***	0.402***
	(0.00282)	(0.00297)	(0.0121)	(0.0190)
Age Square	-0.000489***	0.000137***	-0.00102***	-0.00496***
	(0.0000429)	(0.0000456)	(0.000186)	(0.000286)
Height	-0.000811*	0.00102**	0.00860***	0.00667**
	(0.000414)	(0.000465)	(0.00190)	(0.00288)
Muslim	-0.00779	0.0331**	-0.159***	-0.347***
	(0.0113)	(0.0134)	(0.0524)	(0.0829)
Christian	-0.0132	0.0106	-0.0780	0.240*
	(0.0237)	(0.0226)	(0.114)	(0.131)
SC/ST	0.160***	-0.0199***	-0.135***	0.254***
	(0.00720)	(0.00745)	(0.0288)	(0.0461)
Constant	-0.903***	1.315***	-2.885***	-4.283***
	(0.199)	(0.219)	(0.960)	(1.371)
Sample Mean	0.29	0.74	1.07	2.43
District Fixed Effects	Yes	Yes	Yes	Yes
Observations	19613	19003	19613	19613

Notes:

(1) Data from IHDS 2005

(2) Columns (1) to (4) show IV results using age of menarche as an instrument for age of first marriage.

(3) In columns (3) and (4) use a Z score index.

(4) Robust standard errors in parentheses.

(5) Asterisks denote significance: * p<0.10, ** p<0.05, *** p<0.01